

The Boston Medical and Surgical Journal

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August 31, 1922

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The Massachusetts Medical Society.

PAPERS AND DISCUSSIONS OF THE SECTION OF MEDICINE.

AT ITS MEETING AT THE HARVARD MEDICAL SCHOOL, BOSTON.

June 13, 1922.

CHRONIC SEPSIS IN PREGNANCY.

By JOHN E. TALBOT, M.D., WORCESTER.

The problem of chronic sepsis as an etiological factor in the diseases of the human system is so large that it would be impossible to present it as a single clinical unit. There is in obstetrics, however, an opportunity to study its workings to better advantage because of a less diversified set of manifestations. It is the purpose of this paper to call attention to the presence of a large clinical entity in obstetrics which has chronic sepsis as its initial lesion.

Many cases in obstetrics manifest a persistence of pathology in each successive pregnancy. The woman who has repeated uninduced miscarriages, "the habit of abortion," as it is called, is a well recognized unit. Some cases, however, show a great variety of pathological events.

This persistence of pathology strongly indicates the presence of some continuous etiological factor in the case.

The problem, therefore, becomes one of finding a continuous etiological factor and working out the process by which this etiological factor produces its variety of manifestations.

No one disease entity plays so important a rôle in the pathology of obstetrics as toxemia of pregnancy with or without convulsions.

Early in 1917 I had two cases at about the same time which directed my attention to the association of chronic sepsis with toxemia of pregnancy and eclampsia.

Observations made during the subsequent year convinced me that chronic sepsis was always present in association with toxemia and eclampsia. After writing a paper recording this feature and endeavoring to explain the relationship, I found that La Vake² of Minnesota had written a paper in 1916 to the same effect. Mosher of Kansas City³ has also called attention to the frequent association of chronic sepsis with toxemia of pregnancy. After five years of observation I am still convinced of the constant association of chronic sepsis with toxemia of pregnancy. Here, therefore, is a constant etiological factor of the type which would persist throughout subsequent pregnancies and fulfill the first requirement of the problem.

There are, however, other manifestations of pathology in obstetrics which sometimes occur without the association of toxemia, yet have so much greater incidence in those cases that are toxic that they are believed to be a part of the clinical entity of toxemia. I refer to such

units as antepartum hemorrhage, abruptio placentae, macerated fetus, hydramnios and deformed babies, and premature babies. My own observations would add to this list miscarriages, inanition fever, and hemorrhagic disease of the new-born.

Can all these manifestations be so correlated as to explain their frequent association with each other and with or without the association of toxemia, and lastly with the hypothetical etiological factor chronic sepsis?

The most constant bit of pathology which is found in association with toxemia of pregnancy is the placental infarct. If this so-called "infarct" can be proved to be infectious in origin, the whole problem can be explained.

Young of Edinburgh¹ has shown that infarct can be caused by thrombosis of the maternal blood-vessels of the placental site.

La Vake of Minnesota² has pointed out that thrombosis in other parts of the body is accepted as infectious in origin and further suggests that the chronic sepsis which is always found in association with eclampsia may be the source of the infection which results in the thrombosis of the maternal blood vessels of the placental site. His paper published in 1916 is the first so far as I know which places the cause of eclampsia squarely upon chronic sepsis.

Bacteriological methods to prove that the infarct is infectious in origin have not proved successful. De Lee³ isolated a staphylococcus from an infarct in a single case 20 years ago.

The clinical phase of this problem has not been studied before so far as I know.

The sequence between acute infection in the head and miscarriage and threatened miscarriage has occurred with very impressive frequency in my experience. In the last part of 1920 two cases showed evidence that the damage done by the bleeding in threatened miscarriage was recorded on the edge of the placenta in the form of a white infarct and that the position of these infarcts in relation to the base of the cord demonstrated that the infarct must have originated at the time the bleeding occurred. These two cases were reported in *Surg., Gyn., and Obstet.*, under the title, "A Clinical Study of the Placenta."⁴

Since that time I have been able to acquire a considerable series of cases in which the position of the infarct on the placenta has been coincident with a bleeding spell and in many of them there has also been the clinical factor of an acute infection just preceding the bleeding spell. In some cases an acute cold was recorded on the placenta by an infarct contemporaneous in its position on the placenta but without evidence of bleeding. Using this sequence of events it has been possible in some cases to accurately foretell the shape of the placenta and the position of infarcts on the placenta before birth. This prophecy is subject to so many elements of negative error that it can not be advanced as

infallible. Positive results are, however, very convincing.

The frequency with which uterine bleeding in pregnancy is preceded by active infection in the head is only apparent when it is carefully searched for. The relation of cause and effect in this sequence is convincing when it is found that an acute infection is sometimes recorded on the placenta in the form of a white infarct so situated on the placenta as to show that it originated contemporaneously with the acute infection, although there may have been no evidence of external uterine bleeding. I do not wish to imply that every bleeding spell is preceded by demonstrable acute infection in the head nor is every acute infection recorded on the placenta. Bleeding spells unassociated with acute infection have, however, always been associated with chronic sepsis in my experience.

It must be borne in mind that the early infarct is hemorrhagic in nature and that the white infarct is the end-result of this hemorrhagic lesion.⁵

Clinically the uterine bleeding which is associated with the beginning infarct does not come from the hemorrhagic lesion in the placental tissue but from the uterine blood-vessels, since it is the mother who becomes exsanguinated in uterine hemorrhage.

Besides this clinical evidence there is laboratory evidence that the primary lesion which results in the placental infarct is in the maternal blood-vessels of the placental site and is infectious in origin.

A case reported by Cornell and Earle⁶ is extremely important on account of the unique set of circumstances which made the obtaining of the evidence possible. In this case a woman in her sixth pregnancy had a bleeding spell at the second month, immediately following a chronic cold of three weeks' duration. Examination revealed an asymmetrical uterus, and the abdomen was opened on the diagnosis of extra-uterine pregnancy. A bicornuate uterus was found with a pregnancy in one side and a fibroid in the other. A hysterectomy was decided upon and a complete pathological examination of the placenta and uterus was made with the placenta *in situ*.

It is hard to conceive of another set of surgical indications which would lead to a hysterectomy just following a bleeding spell in a pregnant uterus. The case, therefore, assumes a great importance on the question at issue. The pathological report describes "an abundance of necrotic areas in the decidua adjoining the intervillous space. They show profound necrosis centrally, there may be some hemorrhage and there is a margin densely infiltrated with leucocytes. Slides stained with Gram's stain afford some inconclusive evidence of the presence of Gram-positive cocci but the histological appearance of the necrotic areas points clearly to their infectious origin."

To summarize this case we have multiple discrete hemorrhagic lesions of infectious origin in the decidua basalis and in association with a threatened miscarriage which in turn was immediately preceded by infection in the throat.

W. S. Thompson of Johns Hopkins⁹ has reported a case of hysterectomy on a case of complete placenta praevia. The pathological examination was made with the placenta *in situ* and the report contains the following extracts. "At the junction of the decidua with the chorionic membrane there are numerous areas of closely packed polymorphonuclear leukocytes in various stages of preservation which apparently represent minute miliary abscesses. Sections through the posterior wall of the placental site show that the placenta is almost entirely infarcted and presents numerous areas of calcification. Leucocytes extended far out into the muscularis, forming parallel rows between the fibers." This histological examination of the uterine tissues beneath this much infarcted placenta shows marked evidence of long standing infection.

It is my belief that the first case represents the picture of beginning infarction of the placenta; the second is the picture of the end-result.

The infectious origin of the infarct is again supported by the clinical observation that it is most frequently found in association with three diseases—toxemia of pregnancy, chronic nephritis, and syphilis. Toxemia of pregnancy is constantly associated with chronic sepsis; chronic nephritis is today recognized by many as being the result of chronic sepsis; and syphilis is the best understood of our chronic infectious diseases.

The analogy between the results of syphilis in pregnancy and chronic sepsis in pregnancy is most striking.

Before the Wassermann test was widely used, repeated miscarriages were considered to be a symptom of syphilis. Macerated fetus and deformed babies were also believed to be evidence of syphilis. No one doubts today that syphilis is the etiological factor in many cases of repeated miscarriages, of toxemia, of macerated and deformed babies and of premature births. Infestation of the placental site by the spirochaete is known to produce a much damaged placenta and these bacteria often pass through the villous membrane and infect the fetus. Similarly pneumococci,¹⁰ influenza bacilli,¹¹ streptococci¹² and staphylococci¹³ have been found in stillborn and living babies. A case of miliary tuberculosis¹⁴ in a stillborn infant has recently been reported. All this evidence demonstrates plainly the principle of hematogenous infection of the placental site. I believe the situation may be summed up by the statement that everything that syphilis can do in pregnancy can be done by chronic sepsis of other types of bacteria.

The evidence tends to show that such units as threatened miscarriage, miscarriage, ante-

partum hemorrhage and abruptio placae are all manifestations of the same process, differing only by reason of the site of the lesion, the degree of the damage and the virulence of the bacteria causing the lesion. Threatened miscarriage and antepartum hemorrhage from normally implanted placentas represent bleeding from the edge of the placental site. Miscarriages and complete detachment may result from this process but may also result from retro-placental hemorrhage without initial external hemorrhage.

Macerated fetus and deformed babies result either by direct infection of the fetus or by the interference with the oxygen supply of the fetus. Stockard of Cornell¹⁵ has produced twins, double monsters and deformities in fish by reducing the oxygen supply of the eggs at a crucial time in their development. In my series there are nine consecutive cases of deformed babies which were associated with evidence of placental damage at the base of the cord. The relationship of this damage to the base of the cord shows that the initial lesion occurred very early in the pregnancy.

Hemorrhagic disease of the newborn and inanition fever are evidences of infection of the fetus before birth by the presence of an acute infection of the placental site just previous to or during labor.

All these units depend upon infection of the placental site.

There is much evidence to show that toxemia of pregnancy is not the result of infarct formation or the result of infection of the placental site except that such infection may act as a new area of sepsis.

In a paper of this length it is not possible to discuss toxemia of pregnancy in detail viewed from the standpoint of chronic sepsis as the etiological factor. Let me mention in passing, however, that the multiple areas of necrosis in the liver of the dead eclamptic suggest a strong analogy to the multiple necrotic areas found in the placental site of the much infarcted placenta. Emerson of Indianapolis¹⁶ has maintained that chronic nephritis is not a self progressive disease but the result of repeated minute acute infections of the kidney tissue. The analogy between this point of view and the rest of the picture as shown by the much infarcted placenta in association with chronic nephritis and toxemia of pregnancy is likewise most striking.

The degrees of toxemia of pregnancy may be explained by the kinds of bacteria involved in the areas of chronic sepsis and their virulence. This point of view makes it possible to explain why the results of infection of the placental site, such as antepartum hemorrhage, macerated and deformed babies, premature babies, hemorrhagic disease of the new-born and inanition fever are so frequently associated with toxemia

of pregnancy and yet may occur without evidence of toxemia.

The clinical sequence described in the beginning of this paper shows that the source of the sepsis is generally in the teeth or tonsils. Many of these tragedies of obstetrics are, therefore, preventable by the timely removal of these areas of chronic sepsis.

The removal of chronic sepsis in the presence of a pregnancy has, however, very definite limitations. There is a well known saying current among the medical and dental professions and among the laity that dentistry should not be done during pregnancy. I believe this saying is well founded when it is limited to the treatment of septic conditions in the teeth. It is well recognized that miscarriage is most frequent up to and including the third month of pregnancy. Up to that time the placental site is relatively small. A beginning infarct during this time would be more liable to terminate the function of a relatively large part of the placental area. If this damaged area is proportionately large enough it will asphyxiate the fetus by reducing the available oxygen supply and a miscarriage will result. It is, therefore, not safe to take a chance on increasing the activity of sepsis before the beginning of the fourth month.

It is likewise true that chronic sepsis should not be disturbed in the presence of toxemia of pregnancy. It is well recognized that many cases of toxemia are apparently precipitated by acute infection.

I heartily agree with La Vake,¹⁷ who believes that it should not be the duty of the obstetrician to remove chronic sepsis and that the proper time for its removal is either before or between pregnancies.

If chronic sepsis is found, however, in the pregnant woman, experience has shown that the safest time to have it removed is between the fourth and seventh month of her pregnancy.

CONCLUSIONS.

1.—The white placental infarct is the end result of a hemorrhagic lesion, its evolution being described as a coagulation necrosis.

2.—The lesion is a discreet process, often multiple and often repeated in the same placenta.

3.—The placental lesion is secondary to a hemorrhagic lesion in the maternal blood vessels of the placental site.

4.—There is clinical and histological evidence that the primary lesion in the maternal blood-vessels of the placental site is infectious in origin.

5.—The clinical sequence of events observed shows that the lesion is the result of hematogenous infection and that the source of the infection is generally to be found in the teeth or tonsils.

6.—By the determination of the infectious origin of placental infarcts, a large clinical en-

tity is demonstrated in pregnancy which has chronic sepsis as its initial lesion.

7.—Treatment by the removal of areas of chronic sepsis in the presence of a pregnancy should be pursued with the greatest caution.

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DISCUSSION OF DR. TALBOT'S PAPER.

DR. S. B. WOLBACH, BOSTON: There are a number of things in Dr. Talbot's paper that interest me. One is that one may time the occurrence of placental infants with something that has happened to the individual during the pregnancy. That seems very important because it gives the obstetrician or clinician an opportunity to test out one of the main points of Dr. Talbot's paper. I believe that it is probable that the growth of the placenta is only by peripheral extension. That has always been my conception, and I have conversed with embryologists in regard to this. When the fetal villi have implanted themselves they stay placed and new fetal villi extend peripherally during the growth of the placenta. An infarct at the extreme periphery of a fully developed placenta can occur only very late in the pregnancy. This can be tested out by anyone in charge of pregnant women.

I think Dr. Talbot should be congratulated upon having impressed that fact upon us.

The circulation in the placenta, of course, is extremely interesting, and the wonder is that accidents don't happen more frequently. Embryologists and physiologists have called attention to these large pools in which the blood bathes the fetal villi and the fact that the villi are not covered with endothelium but with epithelium. Of course, we don't know much about what goes on in the placental circulation, but theoretically it affords a good place for accidents, and from *a priori* reasoning, for the localization of infectious agents. From our work on animals we find that infection of the placenta occurs quite often. In working with organisms of low power of invasion we find that those organisms often invade the placental tissue, while other tissues are not invaded. I became interested in this some years ago, and knew that it is possible to test out some of Dr. Talbot's work experimentally.

The only part of Dr. Talbot's paper I would disagree in any way is the extent of his conclusions. I should think he must present much more evidence be-

for he can prove that hemorrhagic disease of the new-born is due to infarction of the placenta. I deplore that slightly, and also his use of the term "chronic sepsis." I should rather have him speak of *coined* sepsis than of chronic sepsis, and leave the question of the importance of the tonsils and teeth in the same category where we place them in respect to some other diseases of infectious origin, such as chronic arthritis.

I want to impress upon you the strong impression that Dr. Talbot's paper has made not only upon myself but upon some other pathologists, and I sincerely hope that eventually we will learn more about the pathology of the placenta. If practitioners wish help from the pathologist in working out questions of this sort, the initiative must come from the practitioner; and it is desirable that he should do as much of the research as possible, aided by the pathologist.

DR. JAMES R. TORBERT, BOSTON: I have had the pleasure of discussing this subject with Dr. Talbot at prior meetings, and we are agreed. He is enthusiastic about his conclusions for he has done a great deal of work on the subject. He has taken a subject upon which, as you all know, the etiology is more or less in doubt and he has worked it up very well. I am basing my conclusions on the observations of a great many cases at the Lying-In Hospital here in town and also on a great many cases in private practice, and I can't believe that he has proven his point. For instance, we have seen in the last five years 6,000 cases at the Lying-In Hospital, with one case of eclampsia. We haven't found in that list any evidence to account for the fact that there is a focal infection in these cases. We have all the time in the hospital from five to six cases of toxemia of pregnancy, and since 1919, when this paper first came out, we have been looking for chronic sepsis constantly; and we don't always find it. Sometimes we do find it, and I cannot help but believe that there is something else at the bottom of this than chronic sepsis.

Now as to the question of infarcts in the placenta—we have been interested in that to a large extent, and we find a great many instances of infarcts which we can't explain at all. That is, there are no clinical data associated with these cases. Dr. Talbot has seen several of these cases with us and has admitted that he couldn't tell what had happened. I had a case last night in a patient who had a normal child and who had a normal pregnancy throughout, and yet who produced this malformation of the placenta in which we should be able to put our finger on the cause, but there was nothing to be seen in this case. The way I feel about the subject is that this may be one of the causes, but it has not been proven yet; and the fact we have so many cases that show no evidence of chronic sepsis proves the question to be one that is still open for discussion. I have no theory or facts to contradict this other than clinical observation, and I have no idea about the cause of eclampsia. Dr. Wolbach said that when we get up here we will have the assistance of the pathology department and chemistry department and thus we may be able to get a line on the cause, but I have followed Dr. Talbot's paper since 1919 and I am not convinced that there is enough in it to be the actual cause of the condition.

He mentions another subject which we discussed together and that is, the careful observation of these cases. Now, at the Lying-In Hospital it is very difficult to get a definite history of a slight hemorrhage which has occurred during pregnancy. That is perfectly true, we all agree; but on the other hand, where I had been able to get a definite history, I haven't been able to make the facts agree with the pathologist's findings which were present, and I think that there is something else besides this condition which he mentioned as a basis for the various combinations. I feel that my point is very weakly taken, but it is simply the result of observation on a large number of cases, and the only one I have come across where

there was definite evidence of chronic sepsis was one case of eclampsia where there was definite infection in the mouth. In the other cases we haven't x-rayed all the teeth, as Dr. Talbot suggests, but there was no clinical evidence of infection in the mouth at the time of the convulsive seizures or afterwards.

DR. FREDERICK C. IRVING, BOSTON: I am very much inclined to agree with Dr. Torbert on this question. I think Dr. Talbot has made an important contribution to the theory concerning the pre-eclamptic toxemia. In other words, I think his focal infection theory is worthy of taking its place amongst the other theories such as the nephritic theory, the numerous biochemical and serological theories which have been advanced, and the intestinal toxemia theory of the Dublin school. But I agree with Dr. Torbert that I don't think it is the whole story. And the reasons I don't think it is the whole story are these: In the first place, in order to demonstrate anything in medicine or in anything else you not only have to prove that a certain fact is true but you have to prove that the opposite cannot be true. In other words, you have to have controls. Now, if Dr. Talbot has been able to demonstrate that his cases that did not have toxemia or did not have deformed babies or placenta previa or miscarriages, did not have chronic sepsis, then I think we would be more ready to accept his theory. Also there is lacking the bacteriological proof. There is no bacteriological evidence to prove that the same micro-organism which caused sepsis caused any of these conditions. Moreover, as I understand it, in his opinion chronic sepsis is in some way involved in the following conditions: toxemia, placenta previa, separation of the normally implanted placenta, miscarriage, premature labor, deformed children, infant fever and hemorrhagic disease of the new-born. If that is so, it is a unique situation in medicine. Even syphilis, which has so many manifestations, has not so many as this. And it seems to me, to say the least, it is unlikely. Another thing which is hard for me to understand is why chronic sepsis should not be drained during pregnancy. Why, if it is the etiological factor, should it not be eliminated, that is, the sepsis treated under surgical principles? I believe in other conditions where chronic sepsis is believed to be the causative factor, drainage is indicated. At the pregnancy clinic at the Lying-In Hospital we have a class of patients who are not particularly careful about dental hygiene. Their teeth are in an unbelievably bad condition. There must be a great deal of chronic sepsis among them, and yet we find in a series of 5,000 consecutive cases that toxemia occurred in only one and a half per cent. It seems to me if chronic sepsis and sepsis of the teeth are so important, that we would have found toxemia much more commonly than we have.

DR. F. S. DELUE, BOSTON: I believe that there is another factor here other than chronic sepsis. I firmly believe that the process is a nutritional one and that that which causes the focal infection causes the hemorrhage. The condition of the body which robs it of its supply of lime and various other alkalies which neutralize the acids is the condition which gives rise to hemorrhage, whether it be in the eye, skin or placenta or elsewhere. I have found invariably that those cases were cases of primary acidosis, cases in which the lime was evidently deficient in one part of the body or another. Ten years ago I showed Dr. Wolbach a peridental abscess which I told him I could produce at will. In these infections there may be hemorrhage around the teeth, into the anterior chamber, or into the skin, and these are entirely due to improper eating, eating acid fruit being the cause. You have been taught that fruits render the body fluids alkaline whereas it is just the reverse. In using the hydrogen ion concentration test you find that fruits are distinctly acid. When you are constantly using up your lime supply and piling in acid

you get your hemorrhages, due to the alkaline supply being disturbed. We find this condition where there are no teeth and no infected tonsils to act as the focus of infection. The process is an aseptic one; if there are germs present they are secondary and not primary. If you have a rotten tooth or an infected tonsil, take it out; but that isn't the cause of the trouble. That which causes the acidosis causes the hemorrhage, in my opinion.

DR. CRAM: I have a patient who is six months pregnant, about 26 years of age, first pregnancy. X-ray has shown that she has two teeth in the lower jaw which have not erupted, and she has some pus from those teeth. Would you advise having those teeth extracted?

DR. JOHN E. TALBOT, Worcester: I do, as a matter of prevention.

DR. JOHN E. TALBOT, Worcester (closing): It is impossible to take up all the details in the time allowed. One thing is sure. I cannot share Dr. Torbert's views. He shows that he has a mistaken point of view of what I was trying to show. I do not claim that I can explain everything that shows in the placenta. The injury to the edge of the placenta is the type of injury which I am talking about, an infarct on the edge of the placenta. I think if you confine your study to that point, you will see that there is a great difference in the findings. I cannot take a placenta and look at it and tell all that has happened in the past history or whether there was clinical evidence of the damage. Chronic sopsis often works silently, without demonstrable clinical signs. The truth is shown by the demonstrable acute infections and the damage to the placenta contemporaneous with that acute infection.

With regard to the pathological study of the placental damage it seems to me the truth of the situation is to be found in the examination of the placenta *in situ*. The infection is in the uterine blood-vessels, not in the placenta. The infection does not necessarily cross the villous membrane. The infarct is an aseptic lesion as compared to the septic lesion which is in the maternal blood-vessels.

EPILEPSY.

BY D. A. THOM, M.D., BOSTON.

To discuss the subject assigned me by the secretary of your section, in the time allotted, necessitates a selection of certain aspects of the problem which are of particular importance, and a consideration of them in their relation to the subject in its entirety.

No longer do we look upon convulsions, loss of consciousness, and changes in personality, as a disease entity, but rather as a syndrome of some underlying pathological condition.

The old conception of epilepsy and the search for some definite pathogenic factor with a no less definite anatomical location accounts to a great degree for our present-day ignorance regarding the etiological factors producing convulsions and therapeutic measures with which to combat them.

The syndrome which goes to make up the condition which has borne the name of epilepsy is neither constant nor characteristic. The signs and symptoms may be as numerous and as varied as it is possible for any organic lesion or psychogenic mechanism to produce which needs no definite pathology, no particular anatomical

site, no specific toxin or pathogenic organism. The convulsions caused by a lesion of the kidney are quite indistinguishable from those produced by an acute infection. In neither case may they differ from idiopathic epilepsy.

Notwithstanding the fact that it is highly desirable from a scientific point of view and for the good of the patient to look upon the convulsive disorders in this way and to utilize all our clinical and laboratory facilities in an effort to determine the underlying cause, all too frequently we are confronted with a residual which defies this method of scientific approach, and it is this group that we may quite properly term idiopathic epilepsy.

Before we are justified in making a diagnosis of epilepsy, it is necessary at the outset to eliminate the following organic conditions which are accompanied more or less frequently by convulsions.

Convulsions due to Gross Brain Lesions.

1. Paresis
2. Cerebro-spinal syphilis
3. Brain cysts
4. Hydrocephalus
5. Meningitis
6. Trauma
- Tubercular
- Syphilitic
7. Encephalitis
8. Arteriosclerosis (cerebral)

Convulsions of Toxic Origin.

- A. Endogenous—Uremia, Diabetes, Auto Intoxication.
- B. Exogenous—Lead, arsenic, alcohol, strychnine.
- C. Infectious—1. By toxemia. 2. By producing a meningitis.

Conditions other than those mentioned.

Disturbances of the Internal Secretions.
Disturbances of the Cardio Vascular System.

After the foregoing conditions have been excluded, of which either loss of consciousness or convulsions might be a symptom, such as a brain tumor, uremia or a syphilitic meningitis, we are still confronted with perhaps our most difficult task by a group of borderline cases, which seem to lie in that portion of the diagnostic field called No Man's Land. I refer to the convulsions observed so frequently in association with hysteria, and will review the diagnostic points in another part of the paper. Imagine, for a moment, this step has already been carried out and we are left with the residual which represents what we please to term essential or idiopathic epilepsy.

My conception of idiopathic epilepsy necessitates the presumption of an inherent, unstable nervous system, or perhaps it would be best understood if stated in terms of deficiency in the nerve cells and its processes and its inability to resist reacting to stimuli of such quality and quantity that would not ordinarily affect a normal nervous system.

Such a nervous system is able to cope with a limited amount of mental stress and strain quite efficiently. Many such individuals are combatting successfully the every-day problems of life. It is only when the potential epileptic is confronted by circumstances and conditions which require mental stamina or physical endurance in excess of this average, that he finds that his limited reserve has been completely utilized and there is no further resistance to stimuli, regardless of their origin.

The war was an especially good example of a situation which was continually and repeatedly demanding a degree of stability of the nervous system far greater than most conceivable situations, so that many individuals who broke down under such unusual strain, might well have stood the strife of civil life unnerved. There appears to be a limit to the amount of physical strain and mental stress that every individual is capable of bearing, determined not only by the inherent qualities of the individual, but also by the environmental conditions during the formative period of early life.

Some of the theories that have been advanced to account for the convulsive phenomena in idiopathic epilepsy are perhaps worthy of mention. Of the older views and those subscribed to by many authorities of the time, that of *vasomotor spasms* which Tenor and Kusmaul sponsored, was perhaps the most popular, as well as the most plausible, the site of the lesion being the vasoconstrictor center in the medulla. Sclerosis of the cornu ammonis and lesions of the sympathetic ganglia described by Merynert and Echebria respectively, both had many adherents in their day. Of the more recent theories which have been presented for our consideration, one which considered the increased rapidity of the coagulation time of the blood as responsible for convulsive disorders, was brought forth in 1912 by Sprangler of Philadelphia who recommended the injection of snake venom as treatment.

Another theory which attributed epilepsy to an infection due to the specific organism called the *bacillus epileptius* described by Charles Reed of Cincinnati, ex-President of the American Medical Association, received considerable attention about 1915. The psychogenie theory expounded by Dr. L. Pierce Clark of New York has been gaining many adherents during the past eight years. Clark states that "epilepsy is a psycho-biological reaction and is double motivated. First, it seeks to make away with an intolerable adjustment demand; second, to retreat to regress where a state of harmony and peace are wont to prevail," referring to the primitive pleasurable state of intrauterine existence.

Dr. Walter E. Dandy of the Johns Hopkins Hospital, presented two cases early in 1920 of epilepsy that had apparently been cured by a new form of operative treatment, and concluded that epilepsy was due to a definite lesion of the

brain stating, "Am encouraged to a hopeful outlook for its treatment." The diagnosis of Jacksonian Epilepsy was made in the first case which was that of a man thirty-four years of age who had definite localizing symptoms. The second case was a boy eight years of age, also suffering from "focal epilepsy." It is obvious that the two cases presented by Dr. Dandy would not have been included in my conception of epilepsy, but inasmuch as his work attracted considerable attention, it seemed worthy of mention.

Dr. John Mason Little recently published in the BOSTON MEDICAL & SURGICAL JOURNAL, January 19, 1922, a most interesting article entitled "Observations on the Operative Treatment of Epilepsy" with a report of fourteen cases. Dr. Little had no "operative deaths" and felt that no harm was done to any of these patients. He states, "Without exception, there was temporary improvement. There were three cases which can be called cures, over eleven and a half years; eleven years, and five months; and ten years. Three are relieved and living useful and happy lives, nine, six, and five years after operation. My general impressions are," says Dr. Little, "that it has been well worth while." He wisely and cautiously sounds a note of warning against overenthusiastic surgery and indiscriminate operating for epilepsy, yet leaves us with but little doubt that there are certain cases that may be benefited by operative procedure, but they should be selected very carefully after intensive study on each case.

Perhaps the most recent experimental work that has been carried on relative to convulsive disorders, is that which has been stimulated by Dr. H. Rawley Geyelin of New York, relative to the acid secretion during prolonged fasting in epileptics. This is the only report available up to this time. Eleven patients, ranging from eleven to thirty-five years of age, were fasted for periods from fifteen to twenty-one days. The fast was employed as a method of treatment on this group and careful examinations were made to determine quantitatively the following substances in the urine: creatinin, creatin, ammonia acid, titrable acid, total acetone bodies and total nitrogen. Quoting from Dr. Geyelin's paper, "There are five cases which show a relative high excretion of total N/10 acid. These cases also show a high total acetone excretion. The remaining seven cases show a relative lower acetone excretion." The therapeutic results observed seem to be somewhat dependent upon the total amount of acid excreted; that is, in general those patients who showed the highest acid excretion, were those who derived the maximum relief of symptoms. All those patients who showed the lower total acid excretion were not improved symptomatically except during the period of fast.

Dr. Stanley Cobb of the Harvard Medical School has devoted much time to experimental

work and clinical observations on the effects of fasting on convulsions, and will be able to give you a much more intelligent account of what may be expected of this method of treating epilepsy than I, I would say, however, that from the evidence presented, and the time that has elapsed since this experimental work began, we have no more reason to expect this line of treatment to be a panacea for all convulsive disorders, than other measures suggested heretofore, so that up to the present time none of the theories which have been advanced to which scientific research could be applied, have stood the test; either the results described were not sufficiently constant to be conclusive or the same lesions have been found to exist with some other clinical entity.

It is perhaps because so many of these idiopathic cases have their origin in childhood or early adolescence that the question of heredity has received, what I consider, undue importance and has had a tendency to lend an air of pessimism and hopelessness in the treatment of these cases.

In a study begun at the Monson State Hospital in 1916, and completed and read at the American Association of Psychiatry just a year ago this time, I was able to substantiate certain preconceived ideas regarding the transmissibility of epilepsy from parent to offspring.

One hundred seventeen cases were selected where the diagnosis of idiopathic epilepsy had been determined. Each case had been married and was a parent of one or more children. The total number of children resulting from these one hundred seventeen matings was four hundred thirty-one, of which two hundred eighty were still living at the time the study was finished, May, 1921. One hundred fifty-one were dead. To summarize briefly, nineteen of the one hundred fifty-one died of convulsions. Of the living cases, fourteen have had convulsions at some time during their life, but six have had remissions for a period varying between four and twenty-four years. The remaining eight of these offspring are still having convulsions and considered epileptic. Of the entire group, 75% of them were over twenty years of age, the zone where 70% of all convulsions begin.

The conclusions drawn from that study were as follows:

1.—Epilepsy as a disease is not transmitted directly from parent to offspring, but we believe rather that it is the nervous system, lacking in the normal stability, that is inherited, and the manifestations of this instability may be mental deficiency of varying degrees, mental diseases of different types, neurological and psychopathic disorders, and convulsions from various exciting causes, which would have little or no effect on a normally developed nervous system.

2.—These mental and nervous disorders are less frequently found in the offspring of the

so-called epileptic than we have heretofore believed and the future of the offspring born of epileptic parents is not as hopeless as the pessimistic authorities on heredity have heretofore stated.

3.—Maternal defects are more frequently manifested in some form or other in the offspring than are the paternal defects and, when present, are more likely to appear at an earlier age.

4.—It was found that in only a few cases were we dealing with "pure cultures of epilepsy." In most instances contamination was brought about by some defect in the other partner such as feeble-mindedness, insanity, alcohol and syphilis.

5.—In this study it was determined that convulsive disorders were more frequently met with in the offspring of the organic group as compared with the idiopathic group. The organic group was, however, so small that too much consideration can not be given to this point. It should, nevertheless, stimulate further inquiry relative to the offspring of normal individuals and a larger group of organic cases.

6.—This study indicates the necessity of research relative to the transmissibility of genetic defects in both epilepsy and psychiatry. We feel that the dogmatism regarding this aspect of mental diseases has not been justified.

Another important problem relative to the convulsive disorders is the relation between convulsions in infancy and early life and the chronic convulsive disorders of adult life. This problem is worthy of far more consideration than it has received up to the present time.

Our attention is constantly attracted to the cases of epilepsy where the history indicates the first convulsion began during some gastrointestinal disturbance, teething, or at the onset of some acute infection, but with the exception of study made by Dr. John Lovett Morse and reported at the thirty-first annual meeting of the American Pediatric Society, no further researches or studies have been made to determine if any relation exists between the symptomatic convulsions in general and the chronic convulsive disorders in adult life.

Dr. Morse's aim was to determine what proportion of the children, having convulsions, but otherwise perfectly normal, had epilepsy, or developed it later, and to find out if there was anything in the history or the manner of the development of the convulsions to show whether or not there were manifestations of epilepsy, or whether they would be followed by epilepsy later. It appears, from his study, based on one hundred and seven cases, altogether too small a number on which to base any conclusions, that the convulsions associated with spasmodophilia are likely to cease. One case out of ten became feeble-minded. One of the three cases where the infantile convulsions were associated with whooping cough developed epilepsy. Of thirty-

nine cases in which there was a single convolution at the onset of some acute disease, two became epileptic and feeble-minded, a third had petit mal attacks and a fourth it is stated might or might not be epileptic. Dr. Morse concluded that it was impossible to determine from the nature of the earlier attacks, what to expect in the way of prognosis and he further stated that epilepsy is more likely to occur when the cause of the attacks is apparently an injury or severe labor than when the apparent cause is disturbance in the digestive tract. The longer the attacks have persisted, the more probable is the diagnosis of epilepsy. The prognosis in infantile convulsions must always be guarded, in fact, there is no way of telling what the subsequent history is going to be.

Another effort is now being made at the Massachusetts General Hospital to add further light on this subject and it may be possible to draw some conclusions which will be of assistance in making a prognosis as to the future in those cases suffering from infantile convulsions.

The evidence at hand all indicates that nothing should be left undone to determine the underlying cause of infantile convulsions and we already have reason to hope that the more modern methods of studying the biochemical reactions of the body will do much toward the solution of this problem. Clinical experience teaches us that in children, otherwise normal mentally and physically, convulsions persisting over any period of time are quite likely to prevent normal mental development, and that many of our infantile monoplegias, hemiplegias, and paraplegias are the result of convulsions and not a symptom of the same underlying process.

Although we do not understand we recognize that the convulsive habit must be taken into consideration and that heroic methods are justifiable in our effort to prevent the development of a motor pathway which may be too easily stimulated into activity.

Another aspect of this perplexing problem of the convulsive disorders is that of differential diagnosis between the functional and the organic cases and the possibility that the two conditions may exist in the same individual. I am forced to admit that, personally, more confusion exists in my own mind regarding the underlying mechanism, as to whether it be physiological or psychological in this group of cases, than in any other field of neurology.

Notwithstanding the clear, clean-cut differential diagnostic points as pictured in most text-books, there are still many confusing cases which defy our present-day conceptions of either epilepsy or hysteria. The following illustrates this point.

Mrs. C., born in Connecticut, twenty-five years of age, birth and development normal. As a child she was considered unusually bright. When ten years of age she had her first convolution, and attended the clinic at the Children's

Hospital. They were occurring infrequently, once in two or three months, did not prevent her from continuing her education to the second year in high school, when she left on account of an attack of rheumatic fever. The attacks usually occur at night, but since her marriage three years ago, she has had attacks at infrequent intervals during the day. These attacks are usually precipitated by heated arguments with her husband. There is apparently no aura, consciousness is lost completely, she bites her tongue, but does not wet or soil her clothing. Spells last about ten minutes and, from the description, have none of the dramatic characteristics associated with hysterical attacks.

The home situation was very chaotic when patient first came to the hospital, as the patient's father had accused her husband of endeavoring to have her put away in an institution on account of the frequency of her convulsions. Patient's mother-in-law had her child and she was not permitted to see it, and the patient was living with her own mother and her husband was living at his home. In spite of this estrangement, there seemed to be a desire on the part of both husband and wife for reconciliation and the case was turned over to the Social Service. The family difficulties were ironed out and the patient and her husband and child separated from the other members of the family and started to build up a little home of their own. This was in October, 1921. Patient was seen last May 17th. At that time she had not had a convolution and things at home were going much more satisfactorily. The point of interest in this case is the fact that this woman having convulsions, beginning at the age of ten, which were epileptic in character, should at the age of twenty-two begin having attacks precipitated by emotional situations which, from an etiological point of view, would have been diagnosed as hysteria, but from the character of the convolution itself, must be diagnosed as epilepsy.

There seems to be little doubt that convulsions of a psychogenic origin with all the characteristic features of hysteria may occur in an individual suffering from idiopathic epilepsy and I have one case in mind of an ex-soldier who had convulsions following head trauma, who had had three convulsions precipitated by emotional stress. These convulsions, however, were quite the same as our conception of the epileptic phenomena.

I appreciate fully how sketchy this presentation has been but I do not hope to do more than to put forward for the discussion of the meeting some of the present-day views concerning this most baffling condition. The more fundamental problems for consideration, I believe, are as follows:—First, the present-day conceptions of the pathology of the disease syndrome or symptom; second, the importance of hereditary factors; third, the futility of trying to crowd all convulsive disorders under one pathological en-

ity and advocating one specific treatment as a panacea for the condition. These are only a few of the important problems that demand intensive study before we can hope to clear away the present-day confusion concerning convulsions of either organic or functional origin.

DISCUSSION OF DR. THOM'S PAPER.

DR. STANLEY CORB, BOSTON: Dr. Thom has said that I was interested in the treatment of epilepsy and especially in the starvation treatment. The most useful thing that we have learned since 1915 is the use of luminal. Luminal has been of benefit to many patients and in some cases has made their lives practically free from convulsions. We must look upon treatment by luminal as symptomatic. In other words, we merely substitute it for bromides. You may use luminal over long periods without getting untoward effects as in bromide poisoning. On the other hand, you are more likely in the unstable patient to have psychotic upsets from the use of luminal. Also it is unsafe to use in more than one and one-half grain doses, which should not be repeated more than once in 24 hours. In other words, any patient in whom you have to use more than three grains of luminal in a single day is a patient I would be afraid of, because later if you want to stop the drug, you might have a psychotic upset.

The second recent advance is the starvation treatment started by Conklin, an osteopath, some ten years ago. The fast is carried out for from ten to thirty days; the convulsions have ceased during the course of the fast in almost every case. The results of these fasts have in a small percentage of cases been remarkable; some have had no convulsions for months and years. Now that is an interesting result—not that you can use the treatment for all epilepsies, but that it will stop the convulsions on some. The work has been followed up, as Dr. Thom said, by Dr. Geven at the Presbyterian Hospital in New York, and he has had some satisfactory results; but on the other hand, there have been many cases that have not been substantially benefited. We know that we may stop the convulsions of some epilepsies by starvation; if we could learn which cases would be benefited, it would be a useful method. We have so far been unable to learn any criteria by which to pick out those cases.

As to the pathology of epilepsy: Dr. Thom said that if we ruled out brain tumor, syphilis, the trauma cases, the meningitis cases and so forth, we had left a group of cases which we can speak of as "essential epilepsy," and that there were both psychological and organic precipitating factors. These arbitrary classifications are bad. As Dr. Thom said, the one underlying factor is an unstable nervous system, but I see no reason why we should not have that unstable nervous system from syphilis, maldevelopment, old meningitis, encephalitis or polio. As I see more autopsies, I find more organic pathology—an old encephalitis, an old scar or an aplasia. The aplasias show an abnormal placing of the nerve cells or a lack of association fibers, probably inherited; but the amount of old encephalitis or meningitis is larger than I had suspected. As we improve our methods of examination, and if we can obtain autopsies on the early cases, not on the old chronic institutional cases, but the so-called idiopathic sort not having frequent convulsions, I think in these we may find evidence of great importance in relation to what may make the nervous system unstable and allow the factors such as alcohol, focal infection, phimosis, or intestinal upset, to precipitate the convulsions. So when you stop the convulsions in an individual by circumcision or by extracting some bad molars or by a nasal operation, we should look upon the treatment as directed to remove the "last straw" from the patient's back and not as removing the cause of the convulsions.

DR. F. S. DE LEE, BOSTON: Dr. Thom has said that an unstable nervous system is usually at the foundation of these things. I think that is very true. I don't know of anything that will upset the nervous system as much as the ordinary eyestrain, and yet that isn't the whole story. Dr. Gould some years ago in trying this out went through an epilepsy hospital testing all the eyes and got very few results. Eyestrain is only one element. There is a reflex element here, and I think in those cases in which the eye is at fault we have to do with a reflex gastric hyperactivity which in its turn by irritating the phrenicogastric causes the convulsion. I had an interesting case on my farm of a Hollander who had his warning and immediately went into a convulsion if he didn't go into the house for a dose of soda or a glass of milk to neutralize the acid. That may seem strange to many of you. Fifteen years ago I took the son of a Boston lawyer who was having seven convulsions a day when coming under my care at the age of seven years. These started at the age of eleven months. I put him on a diet of skimmed milk and bread without butter. During the winter he gained 9 pounds and his convulsions stopped. He had been operated upon and his skull trephined twice with no relief. I have seen three cases start from eating candy, without a doubt. I usually start my cases by fully correcting all errors of refraction and then attending to the diet.

The boy of seven, above referred to, while being given a bath one night suddenly began to scream. The nurse noticed that he did not have his glasses on and not seeing any other reason for his screaming, placed the glasses before his eyes, whereupon he immediately stopped screaming.

This boy had three degrees of hypermetropia, which I immediately fully corrected. When first brought to me only one-half of the hypermetropia had been corrected.

I was interested in hearing Dr. Thom say that 13 years ago fasting was first suggested by an osteopath. I never have mentioned the name of the old army surgeon, Dr. Dewey, though over thirty years ago he advocated fasting for almost every disease under the sun.

DR. DOUGLAS A. THOM, BOSTON (closing): It is quite easy to find organic lesions of the same type in nearly every case of epilepsy. The question is what is the relation between the lesion and symptoms presented. A few years ago at the Monson Hospital we went over 256 autopsied cases, and it was interesting to note that in the high frequency group, those who were having the most convulsions, we found the more normal-looking brains, and in the group of cases that had fewer convulsions we found the abnormal-looking brains.

I don't think it is time to throw away bromides. Many patients stand bromides better than luminal, and in many cases sodium luminal can be combined with bromide, to great advantage.

THE PHYSICIAN AND THE LABORATORY.

BY FRANCIS W. PEABODY, M. D., BOSTON.

THE important part which the laboratory has come to play in medical science is generally accepted and appreciated, but the relation which it should bear to clinical practice remains to be satisfactorily defined. It is obvious to all clinicians of experience that the laboratory never can become and never should become the predominating factor in the practice of medicine, but it is equally evident that sound medicine cannot be carried on without the support of the laboratory and that in the future the dependence of the clinic on the laboratory will prob-

ably increase rather than decrease. Among the men engaged in active medical practice, however, only a small minority can ever hope to undertake extensive laboratory work in connection with their patients, and the great majority of physicians are and will continue to be confronted by the difficult problem of their relation to this growing influence in medicine. To the teacher of medicine, whose foremost duty is to prepare his students for the practice of the future, the same problem presents itself, for the students must be thoroughly trained in the laboratory methods that will be of practical service, but not burdened with those that are highly specialized or of questionable value.

The leading exponents of clinical laboratory work are the large hospitals—especially the hospitals associated with teaching institutions—and these exert a profound effect on private medical practice, but the conditions existing in them are such as to demand for them a separate consideration. In such hospitals laboratory investigations fall into one of three categories. The first includes those which belong to the field of pure research, their object being to advance the limits of our knowledge of disease. With this we have, at present, no concern. The second consists of those laboratory methods that are applied in order to obtain direct aid in the diagnosis or treatment of individual cases of disease. This often means the use of standard methods of proved and known value—methods which have received general professional acceptance—but in addition it means the use of many methods of possible value, the significance of which needs to be thoroughly tested under conditions favorable for critical control. The trying out of newly advocated measures for the diagnosis and treatment of disease must always be an important function of the larger and better equipped hospitals. Many—indeed the majority—of such methods are found to be unreliable or of little practical value, and after their status becomes established they are discarded. Very rarely a new method withstands the test of prolonged observation and proves to be of such practical significance that it can be properly advocated for general adoption. This type of hospital thus serves as the court before which all such new ideas must stand trial and it is astonishing, if not depressing, to compare the enormous amount of time and labor that is spent in gathering evidence with the comparatively meagre results that pass the tests. The burden added to the hospital laboratories by such work is very great, but the importance of the function cannot be overestimated, for it filters out what is useful and protects the profession from much that is worthless.

The third category under which hospital laboratory work is carried on depends on the fact that every hospital is or should be an educational institution, and one of its primary duties is the instruction of all the members of the

staff in the nature of disease. Many of the laboratory data, therefore, that fill the pages of carefully compiled hospital records, do not have a direct diagnostic or therapeutic bearing on the individual case, but they contribute information which throws light on the pathological physiology and clarifies the disease process. In so far as the accumulation of such accessory laboratory observations is instructive to those who are studying the patients, the work is more than justified, but if, as sometimes happens, particularly with the younger members of the staff, it leads to the idea that all these observations are necessary for the proper diagnosis and treatment of any given case, the result may be most unfortunate. Properly used, such laboratory observations are enlightening and broadening; improperly used, they are blinding and narrowing. The real reason for taking an electrocardiogram on every patient with a cardiac arrhythmia is so that after one has studied the records of a large series of cases, he may understand the clinical manifestations of cardiac irregularities so well that he is able to recognize the type of arrhythmia without the electrocardiogram. His increased knowledge should, on the one hand, emancipate him from the need of the complicated apparatus in most cases, and, on the other hand, help him to appreciate the occasional case in which careful instrumental study is desirable. From this point of view, therefore, much hospital laboratory work may be regarded as of indirect significance for the individual patient, but aimed at the training of better clinicians. When, as sometimes happens, it results in the production of poor clinicians, unable to interpret disease except through the eyes of the laboratory, its purpose has failed and failed seriously.

The physician engaged in the actual practice of medicine is directly concerned, therefore, with only a small part of the laboratory work which is carried on in the larger hospitals, for his attention must necessarily be focussed entirely on those methods which contribute immediately to the better care of his patients. The methods of the teaching clinic cannot and should not be carried into extramural practice. In the hospital all manner of tests can readily be performed in obscure or doubtful cases, but in private practice the economic factor usually restricts one to the tests which most obviously offer practical assistance. Fortunately, however,—and this is apparently contrary to much present-day opinion,—good medicine does not consist in the indiscriminate application of laboratory examinations to a patient, but rather in having so clear a comprehension of the probabilities and possibilities of a case as to know what tests may be expected to give information of value. Even so-called thoroughness should be tempered by reason, and the reason that must dictate the part which laboratory tests shall play in any given case must be the result of a combination of clinical experience with an un-

derstanding of the physiological significance of the available tests.

For the physician in private practice laboratory tests fall into two main classes. The first consists of those which every educated doctor should be able to carry out, and the second consists of tests which are more difficult in technique and which should be attempted only by a limited number of men who have been able to devote the time necessary to acquire specialized training. Fortunately, the first class is by far the more important of the two.

The laboratory tests which should be at the command of every practitioner of medicine are those which deal with the more important and practically useful examinations of the blood, urine, feces, gastric contents, spinal fluids, pleural and ascitic fluids. These are the tests that are customarily taught in the medical schools in the course in clinical pathology, and the instruction is usually designed to take up the laboratory methods that are absolutely necessary for good practice and those only. An experience in teaching this subject during the last seven years has emphasized the striking fact that in spite of the great contributions which the laboratory has made to clinical medicine there has been surprisingly little change in the character or number of the technical methods which are essential for good practice. In many instances the progress of medical science has resulted in a clearer, broader, and more helpful interpretation of the tests, but the actual technical procedures have not been greatly altered and they are still available to the trained man who has a minimum of laboratory apparatus. It has, indeed, been interesting to find how little new material in the way of technical procedure could justifiably be added to the course from year to year, even though the literature and the practices of various clinics were carefully followed in the attempt to keep the course up to date. The methods for the examination of the urine, for instance, are taught much as they were two decades and more ago. Certain tests, such as urea determinations, have been discarded and others are regarded as having a different significance, but the records still show the color, specific gravity, reaction, albumin and sugar content, and the microscopic examination of the sediment. These simple observations, correctly used and interpreted, are practically all that is necessary in cases of nephritis. The modern "two hour renal test" requires nothing more than determinations of volume and specific gravity, and if it is combined with the phenolsulphonphthalein test,—the technique of which is entirely simple,—the field is open for the study of renal function. It is far more important to understand the significance of these easy tests than it is to worry about the quantitation of blood urea or blood uric acid. The situation is much the same with regard to hematology. The technical procedures of primary value are now as

they have been for years, the counting of white cells and red cells, the estimating of hemoglobin, and the preparation of stained specimens of blood. Quite recently the students at the Harvard Medical School have also been instructed in the methods of counting platelets and of staining reticulated cells, but neither of these procedures involves any essentially new technique. With these, and one or two other tests, such as coagulation time and bleeding time, the field of hematology is open. Again, the technique has been altered but little, and little has been added to it, but modern investigations have brought to it a greater significance. In the examination of the spinal fluid the cell count, which is the most important point, is merely an adaptation of the method of counting blood leucocytes, and not a new technical process. With regard to the examination of the gastric contents, body fluids, and feces the same argument holds true; none of them involves difficult or prolonged examinations or expensive apparatus, and all of them yield information of the highest value to the man trained in their use and interpretation. Here, however, is the crux of the situation. All of these so-called routine tests are easy and consume little time in the hands of a trained man, but they are difficult, time-consuming, and of little value in the hands of an untrained man. What is really needed in the application of laboratory methods to the practice of medicine is not a knowledge of more technical procedures, but a much more exact knowledge of a few technical procedures. Experience has shown that a proper degree of technical skill can rarely be obtained during the medical school course, and it should be the duty of every hospital to see that no house officer receives his diploma unless he has demonstrated an ability to perform satisfactorily all the simpler laboratory examinations and has shown a knowledge of how to use the results in the study of his patient. If every physician was so much at home with the technique of the simpler tests that it was quicker for him to apply them than to wonder whether they were worth while applying, and if he understood how to interpret these tests and gain the maximum information from them, the problem of the relation of the physician to the laboratory would be largely settled.

The second group of laboratory methods having a direct bearing on the practice of medicine consists of those which involve highly specialized technique and complicated apparatus. Electrocardiography, basal metabolism determinations, the Wassermann reaction, clinical bacteriology, and the various types of chemical analysis of the blood fall into this category. The information to be elicited from these and other analogous methods is often extremely valuable, but their application is necessary only in a comparatively limited number of cases. As a whole, these methods do not have the broad general sig-

nificance and importance that characterize the simpler tests just referred to. It is, of course, highly desirable that they should be available to practicing physicians so that they may be used in the cases in which they are particularly indicated, but fortunately there is no necessity for the great majority of physicians to bother themselves about the details of technique. This should be relegated to a small number of men who are devoting their attention to specialized fields. Simplified technical procedures, supposed to be adapted to the use of practicing physicians, are continually being advocated as substitutes for the recognized standard methods employed in performing some of these tests, but they are frequently unreliable or reliable only in the hands of one who has a thorough knowledge of all the sources of error, so that it is far wiser to avoid them and to obtain the dependable observations of experts. The clinician may, therefore, neglect the technical side of these more elaborate tests with a clear conscience, but in so doing he should not feel that he may drop the matter entirely. If he is ever to make use of them—and this the welfare of his patients may demand—he must have an understanding of their significance and of the physiology underlying them. He must know when they are indicated and when they cannot be expected to give important evidence. A little insight into the fundamental principles of metabolism, for instance, and a recognition of the common relationship between increased heat production, pulse rate, and certain other symptoms are of the greatest help in deciding in what cases an observation of the basal metabolism may be of diagnostic significance and in what cases it is entirely superfluous. It is much more important to know in what particular case a determination of the basal metabolism may be of value than it is to know the details of the performance of the test. Then again, the physician should be able to interpret the results of the test in the light of his individual patient. A basal metabolism which is reported as 15 per cent. above normal may or may not be significant, and an electrocardiogram showing a prolonged conduction time may be due to one of several factors, but in either case the physician should not be forced to depend for the interpretation on the man who does the laboratory work and who presumably has a less intimate knowledge of the clinical condition of the patient. The clinician himself should be able to appraise the laboratory findings if the patient is to derive the greatest benefit.

It is frequently alleged that many of our medical schools and teaching hospitals are producing "laboratory men" instead of clinicians. If it is true that the graduates of these institutions enter the practice of medicine handicapped by their dependence on the laboratory, then the system of training is wrong or—what seems more probable—it is imperfectly carried out. When schools and hospitals do their full duty their

graduates will have had an opportunity to study disease intensively, checking and controlling their bedside observations by a variety of exact laboratory investigations. Such an experience will enable them to correlate the clinical manifestations of disease with the underlying physiological processes so that they can subsequently understand and interpret disease without recourse to all the laboratory procedures which were necessary in their student days. They will enter practice trained so thoroughly in a limited number of simple technical methods that they will not hesitate to use them, and they will understand all of their significance. They will also know when more complicated tests are indicated and how to interpret the results. In spite of the extraordinary influence which the laboratory has had on the development of medical science there is as yet no cause for the physician to feel that he cannot keep up with the requirements of the best modern practice. All of the more important elements are easily within his grasp. The need in clinical medicine continues to be, not for men trained in many laboratory methods but for men well grounded in a few methods—not for better technicians, but for better clinicians.

DISCUSSION OF DR. PEABODY'S PAPER.

DR. GEORGE R. MINOT, Boston: I think that Dr. Penbody has concisely pointed out the relation of the physician to the laboratory. As he has said, the future development is for better clinicians rather than better technicians. Good technicians are, of course, indispensable. Laboratory findings must be considered simply as additional clinical symptoms. They should be used to assist in, rather than to determine, the diagnosis, prognosis or treatment. We must not forget that the patient is the centre of our professional life, and not some isolated data directly or indirectly referable to the individual.

It is not uncommon to see patients who think they have been thoroughly studied and have obtained an undoubtedly correct opinion because they have had an endless number of tests made and have been furnished with extensive reports, often meaningless. In turn, the physician at times seems to feel that "if all the tests are done" then he has overlooked nothing; such a man often gives little attention to the accuracy of the tests and loses sight of the fact that proper interpretation of the results is fundamental. In other words, in utilizing the laboratory, one must use his mind and not make acceptance of dogma. In obscure cases for study, all forms of tests may be justifiable and desirable, but to undertake these blindly is wasteful and useless. Such a procedure without meditation tends to lead one to not making use of our best methods of diagnosis, namely, what we see upon looking at the patient and what he tells us by his own words, together with well-directed questions on our part.

It may be emphasized that the clinician should know the principle of the tests and the nature of the technique so as to appreciate the possibilities of error. He should realize the difference of one man's analysis and report from that of another. The same is true of a roentgen ray examination. Two men may describe the same shadow in the gall-bladder region in very different terms and yet give it the same significance or they may evaluate the finding quite differently.

Figures tend to lead one to a sense of false security. Clinical information is seldom appraised in figures. Laboratory data, on the other hand, is often

expressed in figures which are apt to lead one to the conclusion that such data are final and judgment is not required. This is incorrect, for judgment is often the sole method of arriving at the figure. Likewise, figures presented by a technician must be judged as to correctness and evaluated by the clinician. One is apt to be satisfied with figures and to believe they express exactness, forgetting that when used for certain laboratory data they may vary widely with technique and that they often express only approximate values.

The laboratory examinations in many instances should be made in the light of the clinical findings. Much better laboratory work will be done if, for example, one submits a blood smear for examination, stating that malaria is suspected, rather than simply requesting examination of the smear. The technician should have information regarding the clinical aspects of the case in order to know in what manner to approach the laboratory problem.

One must bear in mind that positive results usually should have clinical evidence to support them and that negative examinations are to be cautiously interpreted and repeated if other clinical symptoms do not agree.

Laboratory examinations are too often exceedingly simple and yet often neglected. A common one to neglect is that of studying the stool by inspection; an examination from which one often can obtain a great deal of information.

If the laboratory examination is reported on incorrectly, it seldom seriously injures the patient. However, there is at least one instance where it may kill the patient, and that is by an incorrect report on the iso-agglutination tests for the selection of a donor for transfusion of blood. One may select a wrong donor on account of improperly made compatibility tests, and as a result the patient may die. However, the proper selection of a donor does not depend solely on the laboratory and technician. The clinician must determine this, for he must decide whether the donor is healthy or has any transmissible disease, whether he can safely give the amount of blood desired for the patient, etc. Therefore, the laboratory does not do all; the final analysis is the duty of the clinician.

LEAD POISONING.

BY WADE WRIGHT, M.D., BOSTON.

LEAD poisoning is a disease of increasing importance to physicians. The condition is usually of industrial origin and because of the great variety of trade processes in which some form of lead is used, there are many industrial workers exposed in some degree to the poison.

Cases of lead poisoning which cannot be traced to industrial exposures are not infrequent. Seven per cent. of a large group of cases recently recorded were of non-industrial origin. They may be attributed usually to lead piping of water supplies. During recent months a considerable number of cases has apparently originated in the highly specialized lead piping of condensers attached to unlicensed stills.

Studies made in the Industrial Clinic of the Massachusetts General Hospital showed that nearly 2 per cent. of all patients admitted to the Out-Patient Department were employed at work which exposed them to lead. Of this exposed group about 10 per cent. present evidences of industrial lead poisoning.

Traditionally plumbers are especially common among house painters, but of a large group of

cases of poisoning, house painters constituted but 37 per cent. Various other painting trades, as spray painting, dip painting, and automobile painting, are also responsible for lead poisoning.

Scores of occupations may lead to poisoning. There may be mentioned in connection with structural iron work the jobs of drillers, reamers and riveters; with the rubber industry, compounders, mixers and calenderers; with the printing trades, compositors, linotypers, monotypers and stereotypers; and among other trades, plumbers, steamfitters and gasfitters; sheet metal workers, tinsmiths, electric cable splicers, brass moulders, lead glaziers and handlers of insecticides.

Exposure to lead is so common that physicians may well keep it in mind as a possible explanation of clinical findings of obscure origin when such findings are analogous to those traceable to poisoning by lead.

Despite the fact that lead poisoning has long been studied both clinically and by laboratory methods, there does not yet exist satisfactory and conclusive evidence regarding the mode of entrance of lead into the body and regarding its later disposition in body tissues and organs. Lead is probably capable of entering through the skin, but not in important quantities under industrial conditions. It may be taken in through the nose or mouth and swallowed or be drawn into the lungs. Poisoning can be produced by the introduction of lead through either the respiratory or the alimentary tract, but there is good reason for believing that lead by inhalation is more readily toxic than that received by ingestion.

Laboratory evidence concerning this matter is more valuable than that obtainable from clinical observation of cases originating in contact with various lead dusts as it is obvious that much of the dust drawn in through the nose or mouth of an individual may be swallowed. It is notable, however, that trade processes productive of lead-laden dusts are more hazardous than those with which dust is not associated.

There is probably no recognized poisonous agent which is not known to exert its toxic effects in varying degree upon different individuals. Varying susceptibility to lead is strikingly manifested among lead workers. Cases of poisoning have been reported after exposure of but a few days, while many men have worked with apparent safety through long terms of service in contact with large quantities of some form of lead ordinarily considered dangerous. It is probable that this idiosyncrasy may be at least in part explained through careful observation and examination of persons relatively immune to lead. Considerable evidence exists pointing to a much greater susceptibility to lead among women than among men.

One group of cases of lead poisoning studied showed an average duration in trade, at the time of diagnosis, of nine years, two months.

It is not possible to state with accuracy the average period of exposure **prior** to the onset of poisoning, for lead poisoning is usually insidious in its development.

It is apparent that an individual may take into his body considerable quantities of lead and yet present no symptoms or signs which suggest lead poisoning or warrant such a diagnosis. It is conceivable that all persons exhibiting such evidence of lead absorption as a lead line on the gums are in some degree affected by the presence of the metal in the tissues, but as a matter of practical judgment physicians must at present recognize the existence of lead absorption without determinable poisoning. It is an issue of medico-legal importance because of its bearing upon decisions under workmen's compensation acts.

The manifestations of lead poisoning are many and varied. For years the popular medical conception of the disease concerned little more than colic, constipation, wrist drop, the blue line and basophilic stippling of red blood-cells. Though these signs are frequently encountered, they do not fairly constitute a clinical picture of the disease. Few disorders present as diverse a symptomatology as lead poisoning. There is no characteristic syndrome. Certain symptoms and signs are, however, more frequently encountered than others.

Abdominal pain is usually noted. This pain is cramping in character and localized most commonly in one or both lower quadrants or about the navel. It is much less often epigastric. It may be almost constant over periods of hours or be of short duration. It seems to bear no relation to meals. Chronic constipation is reported in most instances. Diarrhea may exist but is rare.

Vomiting occasionally occurs in severe and especially in relatively acute cases. Epigastric distress, nausea and eructations are common. The appetite usually is poor, but failure of the appetite, contrary to a prevalent impression, is not a reliable early sign of poisoning. Loss of weight is of importance. There is often progressive loss antedating the onset of evidence of disease recognized by the patient. A sense of tiredness and general weakness is an almost invariable complaint.

Symptoms of cardio-respiratory disease are generally associated with evidences of nephritis. Arteriosclerosis is said to be caused by lead poisoning. It is certainly often observed, as is also chronic nephritis. An acute nephritis is unusual, but may be observed in cases of comparatively acute poisoning.

Affection of the nervous system is very common in lead poisoning. In many cases there is early impairment of the memory. Disturbances of vision exist, though rarely. Headache and dizziness are often present. Loss of consciousness and convulsions may be caused by lead and usually lead to a diagnosis of epilepsy.

Somewhat indefinite pain in the muscles, as of the thighs or arms and of the back, is common. Pain in the joints, especially of the knees and elbows, is an important and usually early symptom. Gout, associated with lead poisoning, in painters is possibly directly related to the effects of turpentine and has been observed in a small percentage of cases. Bursitis is not infrequent. Numerous cases of elbow pain in connection with lead poisoning have been studied in the Industrial Clinic of the Massachusetts General Hospital. In a few instances the pain was closely localized to the external epicondyle of the humerus, but the majority showed tenderness on pressure over the head of the radius, the site of a bursa described by Dr. Robert Osgood.

Physical examination of cases of lead poisoning may reveal but few definite signs of the disease.

There is customarily some degree of pallor, often of a grayish hue in well marked cases. It is possible that the characteristic pallor is not due alone to anemia but also to vasomotor changes. Marked pallor may exist in the presence of a comparatively high hemoglobin content of the blood. Loss of subcutaneous fat and progressive wasting are usually noted as the disease advances.

The eyes show, rarely, evidence of paralysis of the muscles. Retinal changes may produce the picture of albuminuric retinitis. Eye reflexes are not affected.

A blue line in the gums has been considered by many physicians as evidence of lead poisoning. It is not, however, a pathognomonic sign. Blue lines may be produced by other metals than lead, such as mercury, and even in a lead worker, the blue line of itself signifies only exposure to lead and lead absorption. There can be no doubt that many well defined lead lines are overlooked in routine physical examinations. Gums which upon casual survey show no abnormality may disclose upon careful examination a fine dotted blue-black marking near the dental edge of the gum, a line often rendered readily visible when the edge of the gum is gently rubbed with a piece of gauze to remove epithelium, food particles and pus, and a hand lens employed, with good illumination.

The heart is not involved in most cases of mild lead poisoning. With advanced arteriosclerosis, cardiac changes incident to such a condition may be noted. Slowing of the pulse rate during attacks of colic is occasionally marked.

The examination of the abdomen usually results in negative findings. Tenderness other than that ordinarily related to constipation is not common. Even during attacks of colic pressure on the abdomen affords relief rather than causes pain.

Early in the disease there usually develops weakness of the extensor muscles of the forearm, more marked in the arm most used at work.

Though other groups of muscles may become affected, this antibrachial type is the most common form of paralysis. Complete wrist drop is a classic sign of lead poisoning, but it is very rarely seen at the present time. Paralysis of the Aran-Duchenne type, affecting the interossei and the thenar and hypothenar groups is unusual, as is the peroneal type of paralysis.

The blood picture of lead poisoning is that of a more or less severe secondary anemia, a hemoglobin ranging from 85% down, a red blood count diminished but seldom to less than 2,000,000 cells per cubic mm. Basophilic stippling of red blood cells, while observed in other forms of anemia than that due to lead poisoning, is an important finding, particularly when coupled with other evidences of lead absorption or poisoning.

Though a slight leucocytosis may exist in early cases, it is not strikingly evident during the course of most cases of lead poisoning. Certain writers attach much importance to a relative or absolute increase in the numbers of lymphocytes and large mononuclear cells. A slight eosinophilia is not uncommon.

Examination of the excreta of cases of lead poisoning may reveal the presence of lead in either the stool or urine or in both. In suspected cases it must be remembered that lead in the stool is not absolute evidence of lead absorption. Reports of chemical examination should be regarded as authoritative only when the determinations have been made by a thoroughly competent chemist or technician, for it has been learned that many of the available analytical methods are not reliable when utilized for the determination of lead in organic mixtures such as urine and feces.

The continued excretion of lead is not adequate evidence of continued poisoning, neither is a negative chemical report adequate evidence of the absence of lead in body tissues.

In summarizing the more important elements in the recognition of lead poisoning there may be considered: a history of known or probable lead exposure, evidence of the presence of lead in the body such as a lead line or lead in the excreta, abdominal pain, constipation, anemia, stippling of red blood cells, joint and muscle pain, weakness, and partial or complete paralysis, usually of the extensor muscles of the forearms.

Unless far advanced the prognosis in cases of lead poisoning is excellent for improvement and very fair for recovery. Cases with advanced paresis seldom regain full function, those showing marked mental symptoms with persistent headache do not do well. The ordinary mild ease with constipation, occasional colic and moderate muscular weakness, under proper treatment, recovers completely.

The fundamental treatment of lead poisoning so far as it is rational is based upon our limited

knowledge of the mechanism of the elimination of lead.

Active kidneys and bowels offer the best assurance of progressive improvement in most cases. A useful routine remedial agent is magnesium sulfate, used in small doses over many days, with occasional free intervals. Potassium iodide is considered of possible value but its worth is not proven. Electrolytic baths have been suggested but their effectiveness is questionable.

Severe colic may be relieved by morphine or by the use of amyl nitrite or nitroglycerine. Atropine is also recommended.

Iron is used in cases of marked anemia and various drugs for the palliative treatment of rheumatic pains.

Electrical treatment, massage and exercises are advocated for the restoration of seriously affected muscles.

Most of the lead poisoning which exists is preventable. Many of the trade processes which are hazardous because of the presence of lead could be rendered far less dangerous than they now are. Lead poisoning is not only a common ailment but the prolonged disability which it causes is productive of a lamentably great total of distress and economic waste.

While the safeguarding of trade processes is not a function of the medical profession, the recognition of disease assuredly is. Until physicians recognize in persons exposed to lead the evidences of lead absorption and of poisoning, the dangers of hazardous trade processes will not be appreciated. Only through the help of physicians can susceptible individuals be identified and removed from danger. Only with competent medical counsel can this most important of the industrial diseases be controlled.

DISCUSSION OF DR. WRIGHT'S PAPER.

DR. JOHN A. KEY, BOSTON: Dr. Wright has covered the industrial and clinical aspects of this condition, and at his suggestion I will confine my remarks to the pathological aspects. The literature on the pathology is voluminous, mostly in German, French and Italian, and mostly from the experimental standpoint, because lead poisoning, as Dr. Wright has said, is a disease which disables and does not kill, and autopsy reports are rather meagre. Just as the literature is voluminous, it is also varied: That is partly dependent on the manner in which lead was given, partly dependent on the time in which it was allowed to act, and partly dependent on the tissues in which each investigator was most interested.

In regard to the pathology of the intestinal colic, there is nothing definite known. Various writers have described degenerations in the coeliac ganglion and also in the plexuses of Meierberg and Meissner in the intestinal wall. Other investigators feel that the colic is due to the direct action of the lead on the smooth muscle, causing contraction. Clinical evidence that lead acts on smooth muscle is evidenced by the action of lead on the uterus and the fact that miscarriages and abortions are frequent in women who are exposed to lead.

The absorption is probably nearly always through the gastrointestinal tract. That is true just as much of lead workers exposed to dust as it is in men who are not exposed to dust. The possibility of absorp-

tion through the skin has been tested experimentally, is evidenced by a high reticulated red count or the presence of numerous polychromatophilic cells or stippled cells. If one compares the percentage count of the polychromatophilic cells and the stippled cells with the reticulated cells, the reticulated count is equal to the sum of the other two. That means from the reticulated count we cannot diagnose stippling because it is obscured by the vitally stained network. I regard the stippled cell as an abnormal young cell. It is a red cell in which small basophilic particles are present. These are quite different from the Howell-Jolly bodies which are derived from the nucleus. The basophilic particles are protoplasmic elements, and while they are not confined to lead poisoning, they are nearly always found if a careful search is made. They are also present in pernicious anemia, pseudoleukemia and in almost every secondary anemia of toxic origin and in anemia following hemorrhage in which hemoglobin is retained in the organism. A number of authors state that stippling is uncommon in lead poisoning. These reports reflect on the ability of the man who looked for them. For instance, Schnitter in 1914 studied the blood from 283 cases and in 239 of these he found stippling, but in the last 58 cases in which his technique was improved he found stippling in 100 per cent. He and other writers in the German literature lay emphasis on the fact that in looking for stippling in the blood smear one should beware of a counter stain. They declare that in the Wright stain or in any other Romanowsky stain the eosin obscures the stippling. So they recommend a single stain of methylene blue.

Paralysis is usually manifested in muscles which are used most; that is, a man who uses his thumb and finger muscles exceedingly is more apt to get thumb palsy or finger paralysis than he is to get a wrist-drop. In the muscles affected there is a secondary degeneration and atrophy. In the nerves there is typical Wallerian degeneration; and in the anterior columns of the cord there is a degeneration of the nerve cells. It is questionable which is primary and which is secondary, the peripheral or the central injury. In the encephalopathies autopsy reports show a pale brain which is usually somewhat shrunken. Microscopic sections have shown for the most part only infiltration around the blood-vessels. In a few cases there are reports of degeneration of the nerve elements. In regard to the muscular pains and the arthritis and neuritis, as far as I know, there are no pathological reports. As a whole, the pathology seems to be shown that lead may attack a wide variety of tissues and cause a parenchymatous change which is later followed by fibrosis. The blood changes are secondary anemia which progresses with the continued absorption of the lead. Usually well-defined lead poisoning is present before the blood count goes below three million, and the patient sees the doctor. It may go on to a severe anemia which resembles a primary anemia or it may go on to an aplastic stage. The hemoglobin index is less than one except in severe cases of anemia where it may be more than one. There is usually a mild leucocytosis which is usually more in the nature of a lymphocytosis, there being a relative and absolute increase of the lymphocytes and of the mononuclear elements. Some authors lay emphasis on the lymphocytosis and others on the mononuclear increase. In some cases the polymorphonuclears are absolutely increased. Apparently lead in the blood stream destroys the polymorphonuclear leucocytes and the red blood cells. Evidence of the destruction of the polymorphonuclear cells is seen in the numerous degenerative forms in the smear. Evidence of the destruction of the red cells is seen in poikilocytosis and in anisocytosis, and in sections of the bone marrow, spleen and lymph glands large numbers of fragmented red cells are found in the endothelial cells. The bone marrow is active.

I think the anemia isn't due to a depression of the bone marrow but due to a destruction of the red cells. The active regeneration in the bone marrow

THE TREATMENT OF RHEUMATIC FEVER.

BY HOMER F. SWIFT, M.D., NEW YORK, N. Y.

[From the Hospital of the Rockefeller Institute for Medical Research, New York City.]

THE treatment of a sick patient depends largely upon the physician's conception of his disease. This is especially true if treatment consists not merely in alleviating pain or other unpleasant symptoms, but is directed towards increasing the patient's power to master the malady. It is well to recall that in all general infections there is a constant struggle between patient and infectious agent, and that recovery is usually brought about by an increase in the defensive mechanism of the host, while a downward course is evidence of a failure of these defensive agencies to keep pace with the inroads of the parasite. At times the struggle between these two antagonists is quickly terminated; the outcome is soon known; either recovery or death closes the picture. We witness an acute disease. Another degree of intensity gives us a chronic infection; the host has not sufficient strength completely to subdue the parasite, but the constant cohabitation of the two contestants is of such nature that the struggle is prolonged: on the one hand, the various defensive agencies of the body are slowly mobilized so that the parasite is either killed or rendered harmless by encapsulation; on the other hand, conditions may be slightly in favor of the infectious agent and there follows either a continual destruction or an abnormal overproduction of certain tissues of the host. In other instances the clinical picture shows successive periods of advantage

on one side and then on the other; the balance is often so fine that the slightest depressing influence upon the patient may permit the rerudescence of symptoms.

By suitable treatment the pathogenic properties of the virus may be kept at a level so low that ordinary diagnostic measures fail to detect its activity. It is entirely conceivable—even highly probable—that drugs or other parasitistic agents may be sufficiently powerful to depress the virus to such a degree that it may be somnolent but not moribund; withdrawal of the drug may permit it again to resume its nefarious work. It is possible, moreover, that a drug may prevent the virus from acting on one organ or set of organs and still be powerless to suppress its activity elsewhere. Thus the clinical picture of the disease may be so altered that it is difficult or impossible to recognize it as a condition analogous to that presented by an untreated patient.

The consideration of latency of infectious agents is one that would require much time to discuss fully. It is a most important phase of many diseases. Because of an increase of the patient's natural defenses the virus may be encapsulated, but remain living and capable of resuming its pathogenic activity should the immuring tissue be destroyed or other inhibitory agencies be depressed as the result of another disease. Again it is well recognized that a microorganism such as the *Treponema pallidum* may lie latent in tissues for long periods without exerting any demonstrable local destructive action, or inciting the usual cellular response; then, as a result of a lowering of the patient's general resistance, or because of a local trauma, lesions characteristic of syphilis may be produced. Similar examples might be cited in other infections. As one studies his patients over years he is more and more impressed with the probability that recurrences in many diseases are due to renewed activity of infectious agents that have lain latent in the tissues for months or years.

The various possibilities just mentioned must all be carefully weighed in considering rheumatic fever, its complications and sequelae. Unfortunately, we do not know the specific etiologic agent. Our classification of the various conditions included under the term, rheumatic fever, rest therefore upon a study of symptoms and signs, upon the peculiar course of these manifestations, upon the response of some of them to certain drugs, and, finally, upon characteristic histopathological alterations in certain organs. It is possible that our conception of the nature of this disease may be entirely altered by new discoveries; on the other hand, if we may draw an analogy from the history of the study of syphilis, it is probable that the discovery of a definite etiologic agent or the demonstration of characteristic immune reactions would extend the picture rather than alter entirely the notions

already extant. Thus clinical study pursued with new methods as well as old will bring us nearer to a true picture of the relationship existing between the various manifestations in different cases.

In recent years an altered point of view towards this disease is reflected by the change in the name from acute inflammatory rheumatism, acute arthritis, or polyarthritis rheumatica, to rheumatic fever. It is evident that high fever, profuse perspiration, marked general intoxication and acute migratory polyarthritis responding to salicylates do not give us a complete picture of the disease; less obvious manifestations point to involvement of important viscera. For example, we now recognize that certain symptoms and signs, of themselves relatively insignificant, but important when considered in relationship to the general infection, indicate active disease in the heart. These are recurring fever and rapid pulse rate without renewed arthritis or evidence of complications in other organs; precordial pain and tenderness, and areas of paraesthesia in the so-called cardiac Head's zones; dyspnea and orthopnea in adults, and in children an increased respiratory rate unexplained by pneumonia or pleurisy; increased area of cardiac dulness; abnormal cardiac rhythms such as partial and complete heart block, auricular fibrillation, and tachycardia. Daily electrocardiographic studies of all of our rheumatic fever patients have convinced us that these symptoms and signs point to actual disease of the myocardium. Rarely is there instrumental evidence without accompanying clinical signs of cardiac abnormality. It is noteworthy that the majority of our patients minutely studied in the past three years show definite bedside and electrocardiographic evidence of myocardial involvement. If we add to this the long-known tendency for rheumatic fever patients to develop endocarditis and pericarditis, it is evident that most persons suffering from *polyarthritis rheumatica* also have *carditis rheumatica*.

In children and adolescents with rheumatic fever, arthritis is often so slight and transitory that it may be easily overlooked, and the entire picture be one of visceral involvement. Recently a child died in the Presbyterian Hospital with symptoms of acute heart failure. We were unable to obtain a history or other evidence of any of the usual rheumatic manifestations: microscopic examination revealed acute rheumatic myocarditis with many characteristic Aschoff bodies: no endocarditis or pericarditis. There is evidence, therefore, that the virus can attack the heart alone and allow all of the other organs to escape injury. Many children with chorea have no fever, or one only of low grade, and show clinical evidence of active cardiac disease; post-mortem examinations reveal histopathological evidence of rheumatic myocarditis and endocarditis. Others may have, in addition,

characteristic subcutaneous nodules, or there may be only nodules and carditis. Our present belief is that these various combinations of the rheumatic series (Cheadle) in childhood point to activity of the unknown virus in the body just as certainly as if there were a migratory polyarthritis. Other less distinct indications of probable continuing rheumatic infection during childhood are repeated attacks of tonsillitis, enlarged tonsils and satellite lymph nodes, growing pains, and certain cutaneous manifestations.

Failure to recover weight lost during the first weeks of illness or continuous loss of weight are presumptive evidence of persistence of infection. In recent years we have weighed all patients daily, and compared their weight charts with other signs and symptoms. Not only in children, but also in adults have we repeatedly observed that stationary weight below that usual for the patient is attended by slight fever, recurring arthritis or evidence of carditis; relapses have usually been heralded or accompanied by additional loss of weight or by a cessation of increase ordinarily evidenced by a recovering patient. A steady gain in weight does not necessarily mean that the patient will not suffer a relapse, but the liability is much less than in the presence of a subnormal, stationary, or falling weight. In this respect, rheumatic fever may be compared to tuberculosis. It has long been recognized that the acute stages with high fever and painful polyarthritis are attended by a rapidly developing anemia and emaciation. Clinicians who studied the disease before the introduction of salicylates describe the starved appearance of rheumatic fever patients who remained in the hospitals for months. Fortunately, the drugs at our disposal prevent most patients going into such a cachetic state; but the majority of them display a similar tendency which is masked unless we follow the weight curves. The loss of weight may be due to several causes: Du Bois and his co-workers have shown that fever *per se* increases the basal metabolism in a very definite manner. In severe infections the toxic state leads to a rapid destruction of body protein. A rough parallelism between the level of basal metabolism and the pulse rate has been demonstrated by several observers. Frequently the pulse of patients with rheumatic fever remains rapid after the fever has disappeared. In many instances this persistently high pulse rate is evidence of a continuation of myocarditis, and suggests that the virus is still active. Thus we may regard a high pulse rate as an indication for rest, and also for increasing the patient's food intake.

Leucocytosis is another important sign of continued infection. It has long been known that leucocytosis was a feature of this disease, and that invasion of new joints or organs was accompanied by an increase in the number of white blood cells; there are numerous observa-

tions that the number of leucocytes falls after the exhibition of sufficient salicylate to eliminate the arthritis and fever. Lately we have been making leucocyte counts of all our patients several times a week, and have been impressed by the fact that after the initial fall in leucocytes accompanying the administration of salicylates or neocinchophen, there is often a recurring leucocytosis which persists in spite of continuing the drug in sufficient doses to keep the temperature normal and the patient free from pain. In order to test the meaning of this sign, patients have been treated in different ways and the result noted.

Our studies now indicate that leucocytosis is an evidence of a persistent infection, and also confirm the opinion derived from other observations that the salicylates and neocinchophen do not completely destroy the virus, but suppress it sufficiently to relieve many of the characteristic symptoms of the disease.

This leads us directly to a consideration of salicylic acid with its numerous derivatives; later, compounds of phenyleinchoninic acid will be discussed.

From the clinical viewpoint probably no more striking therapeutic effect has been seen than that following the introduction of the salicylates for the treatment of acute articular rheumatism. The results were marvelous: intense suffering was relieved, and the distressing sequelae incident to continued high fever and prolonged intoxication were eliminated. The failure to elicit a similar response in patients with other conditions led to the opinion that salicylic acid was a specific for acute inflammatory rheumatism. The activity of synthetic chemists has resulted in numerous derivatives: drugs less irritating to the gastro-intestinal tract than the crude salicylic acid or sodium salicylate, but all dependent upon the salicyl ion for their specific action. Pharmacologists have shown that in common with many coal tar compounds, the salicylates have an antipyretic action due to increased heat elimination. They also are analgesics, they lessen pain in many conditions, both non-rheumatic and rheumatic. But the disappearance of the signs of the inflammation,—swelling, heat, redness, and tenderness,—so strikingly evident in the joints of rheumatic fever patients following the exhibition of large doses of salicylates, is not seen in patients with arthritis of known bacterial origin, nor in animals with experimental arthritis. The so-called specific action of the drug is, therefore, unexplained. This is not surprising when we realize that the etiology of rheumatic fever and the exact nature of the arthritis are unknown. Certain observers have claimed that the antiphlogistic effects of salicylates demonstrated in the arthritis of patients with acute rheumatic fever are not specific. By means of special charts, whereon all of the symptoms and signs of inflammation in each joint are noted daily,

we have recorded in many cases the results following the administration of therapeutic doses of salicylates; many of the patients have been observed for varying periods before administering the drug; the evolution and resolution of the inflammation in each single joint and in all of the joints have been followed; repeatedly we have recorded the sudden disappearance of inflammation in recently invaded joints and as striking a cessation of the tendency for new joints to be involved. At the same time we have failed to observe a similar antiphlogistic effect in patients with gonococcal rheumatism, or arthritis due to other bacteria; patients with serum disease arthritis, likewise, have not been relieved by large amounts of salicylates. These observations are not unique, nevertheless they are of distinct value because made with a definite point of view: to prove the specific antiphlogistic action of salicylates upon the arthritis of rheumatic fever patients.

The therapeutic and toxic doses of salicylates approximate one another. Failure to recognize this fact often results in failure to allay pain. Frequently we have admitted patients who have been taking 0.6 to 1.0 gram (10 to 15 grains) doses of sodium salicylates or aspirin three or four times daily with only partial or no alleviation of their symptoms; subsequent rapid administration of the same drug to a point just below toxicity has afforded complete relief.

The physician should be thoroughly familiar with the symptoms and signs of toxicity: tinnitus aurum, deafness, nausea, vomiting, flashes of light; in extreme instances, delirium; injury to the kidney resulting in hematuria and diminished renal function are occasionally seen if the drug is given too freely. Hanzlik and his co-workers have laid special emphasis upon renal poisoning, and claimed that many of their patients had a temporary gain in weight after receiving amounts of sodium salicylate sufficient to relieve their arthritis and fever. We have repeated these experiments but failed to observe such marked evidence of renal injury, either in the form of severe albuminuria, cylindruria, hematuria, or evidence of an accumulation of edematous fluid in the patient's subcutaneous tissue. Gain in weight in practically all instances could be explained upon the basis of replacement of water previously lost by excessive diaphoresis. Rarely, when salicylates have been given in large amounts, we have seen edema of the extremities and a sudden increase in weight; but in such cases it was subsequently evident that too much had been administered. Our rule is to prescribe the drugs in 1 to 1.5 gram (15 to 22 grains) doses every hour until pain is alleviated or the first toxic symptoms appear—usually tinnitus or nausea; ordinarily from 6 to 12 grams (90 to 180 grains) are sufficient. The drug is then discontinued until the following day when a total amount of from one-half to three-fourths of the toxic dose is ordered.

Depending upon the severity of the case and the therapeutic results the drug is continued in this quantity for longer or shorter periods, and subsequently the daily total is slowly reduced. We have found that an hourly chart on which are noted the therapeutic and toxic effects of the drug for the first two days, and a daily chart on which the joint symptoms, temperature, and pulse rate are recorded have been of great value in helping us determine the correct dosage for each patient.

Individualizing in the administration of salicylates to rheumatic fever patients is most important. No absolute rule can be given except that of studying each patient during each course of treatment. We have seen the toxic dose vary between 6 and 10 grams for the same individual at different times. An even wider range is seen in different individuals. Men, as a rule, tolerate larger doses than women. The body weight of the patient probably bears some relation to the toxic and therapeutic dose.

The beneficial effect of practically all of the numerous derivatives of salicylic acid is more or less proportional to the salicyl ion content. Improvements in forms of the drug concern themselves mainly with compounds that are less irritating to the gastro-intestinal tract. Crude salicylic acid is most irritating and practically never given; sodium salicylate is less irritating but often is repulsive because of its peculiar taste. Acetylsalicylic acid having a sour taste is often better tolerated. The list of salicyl derivatives is too long to describe in detail, and is probably much larger than necessary. Numerous preparations have been introduced to the profession by enterprising drug manufacturers with glowing testimonials. Had physicians been well acquainted with the technique of using sodium salicylate and acetylsalicylic acid properly they would rarely have been compelled to resort to more unusual and expensive derivatives of salicylic acid in the treatment of patients suffering from rheumatic fever. There is no doubt that certain patients can tolerate therapeutic doses of acetylsalicylic acid when they cannot bear sodium salicylate; the reverse is also true. Cases are also seen where other derivatives appear to be better tolerated; but often it will be found that the apparent diminution in toxicity is due to a lower salicyl content of the preparation in question; and the therapeutic effect is proportionally less.

Attention has already been drawn to certain actions of the salicylates. It is important to realize that we do not know the real reason for the so-called specific effect, although there has been much speculation and some experimentation on this subject. Hanzlik and his co-workers have shown that the salicyl ion does not exist in any greater concentration in the joint fluid than in other tissues, and also that it probably does not exist as free salicylic acid in arthritic exudates. Boots and Cullen have found that the

reaction of joint exudates from patients with rheumatic fever is always alkaline, so that free salicylic acid cannot exist in these exudates even though large amounts of salicylic salts were present. The anti-arthritis effect cannot, therefore, be due to free salicylic acid.

Some experiments of mine indicate that during immunization animals receiving sodium salicylate in doses comparable to the largest therapeutic doses for patients produce somewhat smaller amounts of immune bodies than do controls similarly immunized but not receiving salicylates. This suggests that possibly this drug acts by depressing the susceptibility of the animal to the antigenic stimulus of the foreign protein—both bacterial and non-bacterial. In another series of experiments by Boots and myself, salicyl treated rabbits with experimental arthritis induced by intravenous inoculation of green streptococci had fewer purulent joints than did a corresponding number of non-salicylated controls. Both groups of animals had a similar number of inflamed joints; the smaller amount of purulent arthritis among the salicyl treated animals suggests that in some instances, at least, the drug decreased the irritating power of the bacteria in joints even though the joints were infected. This suggests that the action of salicyl in the animal body may be bacteriostatic even if it is not actually bactericidal.

Such an action corresponds with the observed effect in patients with rheumatic fever. The persistent leucocytosis, the continuation of signs of myocarditis, the development of endocarditis and pericarditis in patients under the influence of salicylates, and the liability for many patients to have relapses when the drug is reduced too rapidly or discontinued too soon, all indicate that its chief action is to reduce the intensity of the pathologic process rather than to destroy completely the virus. It seems to us that this reduction in intensity of the disease relieves pain, spares the patient, and permits a more rapid convalescence.

Such effects might lead to harmful therapy if not well understood and considered in the general management of the patient. Relieving certain distressing symptoms and lessening others so that the patient feels almost well may lull both the sick man and physician into a false sense of security. Very frequently I have seen patients who, while taking 2 to 5 grams (30 to 75 grains) of sodium salicylate or aspirin daily have been discharged from a hospital, and after a few days at home without the drug have developed relapses of varying intensity. According to our present opinion such patients were suffering the entire time from the disease, but were unconscious of its ravages; they were, therefore, subjecting themselves to stress and strain beyond a proper point, and losing the benefit of the time already spent in the hospital, and increasing their liability to serious visceral complications. Under such circumstances it is

questionable whether the administration of drugs may not do more harm than good. This statement is not an indictment of anti-rheumatic remedies but rather a common method of using them.

Recently phenyleinchoninic acid and its derivatives have been introduced for the treatment of rheumatism.* Derivatives of quinolene, they were first used for the treatment of gout. Although unrelated, both salicylic acid and phenyleinchoninic acid have been found to increase the permeability of the kidneys for several non-protein nitrogen waste products; both are antipyretics, and both decrease the signs of inflammation in the joints of rheumatic fever patients. We have found that einchophen and neocinchophen act against the signs and symptoms of rheumatic fever in much the same manner as do the salicylates. Cinchophen is liable to induce severe gastric distress in patients taking enough to relieve their arthritis and pyrexia. Neocinchophen, on the other hand, has been free from this unpleasant by-effect, and has been given in daily total doses of from 0.1 to 0.15 of a gram per kilo body weight without severe demonstrable toxicity. All of the remarks already made in reference to the therapeutic action of the salicylates can be applied to drugs of the cinchophen series: they combat certain unpleasant symptoms, but clinically do not seem to destroy the virus completely, as many patients have continued to have a leucocytosis while under their influence and others have developed relapses upon discontinuing the drug. Similar to our experience with both sodium salicylate and aspirin, we have occasionally seen patients continue to develop new symptoms and signs while taking therapeutic quantities of neocinchophen, and experience marked relief when another anti-rheumatic remedy was substituted. Neocinchophen is a useful adjuvant to our therapeutic armamentarium, but up to the present has not been proven a substitute for the salicylates. Each drug at times is useful in replacing the other, when the one first tried is either too toxic or fails to induce the proper therapeutic effect.

No satisfactory serum has been devised for the treatment of patients with acute rheumatic fever. This is not surprising in view of our ignorance of an etiologic agent with which to immunize animals. It now seems probable that the favorable results from anti-streptococcus serum injections reported several years ago were similar to the recoveries seen following the injection of other forms of foreign protein.

Patients with all types of arthritis have been treated by intravenous or intramuscular injection of killed bacteria, various colloidal substances, or other foreign proteins in various forms. Typhoid vaccine seems to have been employed more than other foreign proteins. In

*Atophan was the name first used, but during the war the term cinchophen was adopted. Similarly, neo-atophan was changed to neocinchophen. Another trade name is tolysin.

order to obtain satisfactory clinical results it is necessary to induce a severe chill, followed by fever, symptoms of marked general intoxication and to stimulate a high degree of leucocytosis. Similar treatment has been employed in most general and local infections as well as in the treatment of arthritis. It is now recognized that there is nothing specific in this form of therapy; that when improvement follows it appears to be due to a sudden mobilization of leucocytes, and possibly other defensive agencies. Many observers who claim the most favorable results from intravenous injections of bacterial vaccines into rheumatic fever patients report only an improvement or disappearance of the arthritis, and an antipyretic effect; they do not mention whether the other symptoms of the disease disappear or not. All agree that the toxic reaction is too severe to warrant the general introduction of this form of therapy, and advise that it be reserved for those patients who do not respond favorably to the salicylates. We have not attempted to repeat the observations of others because we felt that because of the non-specific nature of the reaction it would not help greatly in the elucidation of our problem of the nature of rheumatic fever, especially when such marked benefit can be obtained from drugs.

No discussion of treatment would be complete without mentioning the question of tonsillectomy or the removal of other foci of infection. There can be little doubt that there is a relationship between tonsillitis and rheumatic fever; the exact nature of that relationship is another question, and one we shall not take up at this time. The proper removal of diseased tonsils is often followed by marked improvement in patients who are suffering from chronic forms of this disease. Even from the most conservative point of view, in which it might be claimed that tonsillitis is not "rheumatic" in nature, it would seem advisable to remove simple diseased tonsils, or tonsils that were the point of origin of other general infections. Even more, then, would one advise tonsillectomy if he believes that diseased tonsils are directly responsible for an attack of rheumatism. It is, however, well to bear in mind that many relapses occur in individuals who have had their tonsils well removed, and to warn patients of this possibility in advising operation.

Summary: Within the limits of this paper it has been impossible to consider the treatment of the various important complications of rheumatic fever, or to give specific directions for the use of the remedies discussed. An attempt has been made rather to summarize our present conception of the disease as an infection lasting longer than is ordinarily believed, to show that only certain of the well-recognized symptoms yield readily to anti-rheumatic drugs, and that these drugs often simply depress or mask the activity of the virus. In general, it is important to recognize what our remedies fail to do as well

as what they do. Proper treatment consists not only in administering anti-rheumatic drugs, injecting foreign protein, or removing possible points of focal infection, but also in prescribing rest, guarding the heart against over-strain, regulating exercise, attending to the diet, and in devising measures to increase the patient's general resistance against infection.

DISCUSSION OF DR. SWIFT'S PAPER.

DR. ROGER L. LEE, Cambridge: I think we will all agree that we have had a splendid paper that has given us an adequate résumé of the treatment of rheumatic fever. What Dr. Swift didn't say—he was too modest to say—was that many of his statements are completely backed by his own experimental work in the laboratory. Dr. Swift is combining his clinical studies with his laboratory studies, and it is a piece of work that represents the best that there is in internal medicine. To some of us it is particularly gratifying to find that his carefully made conclusions agree with our own less carefully made conclusions at the bedside. For example, it has always been a pet phrase of mine that there was no specific in rheumatic fever, that salicylates weren't a specific, but that rest and salicylates properly given came near being a specific in rheumatic fever; and I think that Dr. Swift has come to practically the same conclusion. We are dealing with a self-limited disease, although its self-limitation isn't so sharp as in the infectious diseases. We have the asset of self-limitation, we have the asset of salicylates and we have the asset of rest; and these three assets give us in a measure a control over the disease.

I liked Dr. Swift's insistence on the point that after you have apparently aborted the disease, you were still on very dangerous grounds, and I would like to emphasize, as he has emphasized the salicylates, the importance of rest in preventing relapses. It has been our experience in the hospital that patients who have come in after taking the salicylates up to the toxic dosage without much benefit are often promptly relieved under hospital conditions, where rest can be made nearly absolute.

I was also glad to find that Dr. Swift in his studies had difficulties with the obstinate case. That agrees with our phraseology of "almost a specific," because there are cases that are obstinate under the best of treatment, and those cases do not seem to give way to any form of therapy. However, a properly applied therapy will minimize those, and we only have the inevitable case that occurs in every infection.

A MEMBER (Lady): I would like to ask Dr. Swift what he does when sodium salicylate produces nausea.

DR. SWIFT: We have regarded nausea following large doses of salicylates as central in origin. Some patients, on the other hand, find sodium salicylate repulsive because of the sweetish taste; in such cases, switching to aspirin is of benefit. Occasionally a patient is seen who cannot take more than ten to twenty grains of either drug without nausea, or other toxic symptoms; in such cases neo-cinchophen is most useful.

A MEMBER: Do you employ salicylic therapy when there is endocarditis, without joint symptoms?

DR. SWIFT: If there is simply a low-grade fever we do not; but if the fever is sufficient to depress the patient we use salicylates; if he is helped, the drug is continued; if not, it is stopped.

DR. F. S. DE LEE, Boston: I would like to ask Dr. Swift what the condition is of an individual who has rheumatism, recovers without drugs and on taking that again which was thought to be the cause of the

rheumatism, the rheumatism lights up again—whether the rheumatism was lighted up by improper eating, whether the food was the entire cause and whether the individual on shutting off his salicylates and returning to his home did not produce rheumatism by the first cause. I presume you consider every case of rheumatism infectious?

DR. SWIFT: That is my conception of the disease. This conception of the disease is open to alteration at any time sufficient positive proof is presented: I do not regard complete positive proof, however, as arising from a single case, but from a series, because the infection produces so many variations; from a three day disease to one lasting three years. The citation of any one individual to prove a point is hardly scientific; there should be enough cases to prove the contention.

DR. DE LUE: Another question, about growing pains in rheumatic children—that it isn't necessary to have any fever. Would you consider the ordinary stiff neck, without fever as rheumatism?

DR. SWIFT: No, unless they had rheumatic fever before. I have, however, seen patients with other signs of activity of rheumatism, without fever.

DR. DE LUE: You may have rheumatic hearts without fever?

DR. SWIFT: Yes.

A MEMBER: Dr. St. Lawrence regards a level of 99.4 F. as normal; if it is below this point he regards the patient as afebrile and ready for discharge; what is Dr. Swift's practice?

DR. SWIFT: We consider 99.6 F. rectal temperature, as normal, as we have found that mouth temperatures are unreliable, especially if the patient has a weakened myocardium.

A MEMBER: Dr. Swift touched on the matter of tonsillectomy. I would like to ask if he takes out the tonsils routinely and if he does not, what are the criteria for removal of the tonsils?

DR. SWIFT: We do take out the tonsils routinely, because it seems to be the accepted practice. Some patients refuse operation and hence serve as controls. But we explain to all patients that tonsillectomy is only a part of the general treatment and that there is only a possibility of relapse.

DR. E. H. PLACE, Boston: I would like to know if Dr. Swift takes out the tonsils during an active episode, if so, whether there has been any unfavorable reaction; and whether there has been any lighting up shortly after the operation?

DR. SWIFT: I am glad that question has been brought up, because of our experience with relapses following tonsillectomy; although at present unable to give exact figures, I should judge that somewhere between 20 and 35 per cent. of our patients had shown signs of arthritis shortly after tonsillectomy and adenoidectomy. To evaluate this evidence properly, a similar series of patients should have undergone some other operation, in order to see if it is not merely the depression of operation that has brought about the relapses. Most of our patients have lost from three to nine pounds following tonsillectomy; this loss has occurred in spite of special effort on the part of our nurses to keep up a high caloric diet. I am of the opinion that a comparable loss of weight from any other cause would be followed by a certain number of relapses. In other words, many of our patients at the time of operation were harboring the virus, and the operation was the depressing factor that allowed it to become active.

A MEMBER: I would like to have Dr. Swift answer

Dr. Place's question whether he operates during acute symptoms.

DR. SWIFT: No; we have not; in view of the experience just cited we have felt it better to wait until the signs of activity had disappeared and the lost weight recovered. In this manner we hoped that the depressing influence of the operation would be less deleterious.

Original Article.

THE STING OF THE SEA-NETTLE.

BY DOUGLAS H. STEWART, M.D., F.A.C.S., NEW YORK.

UPON page 158 of Weils' Chemical Pathology will be found the following sentence, "Many coelenterates produce active poisons, which have a paralyzing and a local irritant effect." It is to one of those coelenterates, named the sea-nettle, that attention is here called; because at the present time all the animals of the jelly-fish tribe are unusually abundant off the neighboring coasts (July, 1922). For reasons that we might describe as diplomatic the effects of the stings of the sea-nettle are Bonifacially classed in the "Ivy Poisoning" group, although in many instances the plants that produce such "poisoning" may not be within miles of the sufferer. Then, too, the pathological results and the clinical appearances of a "Dermatitis Venenata" differ materially according to a plant origin or that from the sting of a marine animal. Yet at the same time there are sufficient common manifestations to deceive the hasty and inattentive observer in his differentiation.

Information pointing to some sort of sting, experienced by some one who was bathing in salt water at the time, will suffice as an item with which to start a clear medical history.

Unfortunately for accuracy a child-victim will state that he has hurt his knee (for instance) when he has neither knowledge nor evidence of blow, contusion or fall; though he is endeavoring to say in his own way that something happened, so that his knee does not feel nor work as it should. Since the aforesaid was written the writer's attention has been called to two cases of adults who were stung while swimming under water. One was slashed across the mouth and chin and his rash would have been taken for a very severe and extensive grade of herpes febrilis. The other patient was slashed across both cheeks and nose and across the shoulders and back, with a rather prevailing erysipelatoid appearance to the wounds.

It would appear probable that the location of the sting mark depends upon the position of the victim's body when he is stung, i.e., whether he is standing, sitting, wading, swimming, diving, etc. All that is emphasized in this place is that granted a good contact with the tendrils of a healthy sea-nettle and the animal

may be relied upon to attend to the business of stinging. The poison of the *Rhus* is a glucoside or toxicodendric acid, while the poison of the sea-nettle is akin to, or identical with, formic acid, though mixed with other substances. The *Dermatitis Venenata* of the *Rhus* presents redness, swelling and sometimes a diffuse edema; while the *Dermatitis Venenata* of the sea-nettle's sting produces a condition that might be considered to resemble a dermato-cellulitis. It is, however, really an edema the edges of which are as clearly drawn as though marked out by a pencil. Or those edges are as well defined, to the observer's sense of touch, as are the margins of a piece of slate. Blistering, herpes, etc., are seen in either or both forms of *Dermatitis*.

The marks that the stings leave upon the skin of the victim are often found upon the outside of the lower extremity where they run or trend more or less vertically from the middle of the leg to the middle of the thigh. This is theoretically accounted for by the animal's body coming in contact with the upright body of its victim and, since the stinging tendrils hang more or less directly downward in the water, they come into good contact with the outside of the victim's leg. On the contrary should the animal's body be pushed aside by the hand as in the motion of swimming, or should that body come into contact with more or less protuberant or projecting abdomen or hips then the tendrils will swing free without contact, and no sting ensues. When stings are scored upon the arms or face the actual contact areas are often small; hence the amount of damage is limited, or trivial and is prone to be herpetiform in character. When, however, the contact-area runs from ankle to hip, then the irritative result that is provoked is very evident to all concerned.

The writer's observations lead him to think that the long, broad mark often found upon the leg is the result of contact with two or three tendrils; while an arm, for instance, might come into contact with but a portion of a single tendril. Therefore the mark found upon such an arm may resemble a welt, wheal, urticaria or herpes of small circumscribed extent; while a similar mark upon the leg is a more or less perfect rectangle, one or two inches in width and from six to eight inches in length. Upon one occasion several physicians were asked to furnish an appropriate description of the appearance left after a severe sting. The opinion was that it most closely resembled a commencing cellulitis (pulse and temp. normal). After two days the resemblance was lost, as the skin became dark brown in color and the corneous layer was destroyed quite as though there had been a second degree burn or a scald with a blister.

Children appear to suffer the most from after-effects possibly because of tender skins and because the poisonous dose is large in proportion

to body-weight. If lameness be manifested it will not amount to more than the difficulty in walking that follows ordinary contusion, for it has nothing to do with the actual condition of the articulation itself. The severity of result depends upon the condition of the victim and of the sea-nettle. A well tanned, thick, weather-beaten skin furnishes a high resistance; and temperature of water and the usual laws, that apply to greater or less activity of venoms in general, probably have their effect on the animal's efficiency.

The most commonly employed (and perhaps the least valuable) applications to a formic acid sting are the Tincture of Iodine and ordinary Alum Acetate solution. The most popular, most efficient and perhaps the most dangerous in its possibilities is a plastering of clay mud. Nothing better fills all the therapeutic indications than a compress kept constantly moistened with a two per cent. solution of Bicarbonate of Soda. A two per cent. solution of Phenol may be painted on for its analgesic effect, may be allowed to dry uncovered and then the Soda Bicarb. compress may be employed over all, without fear of producing the well-known carbolic gangrene. In short, the attendant will do no damage if he bears in mind that the main object in this instance is to neutralize an acid with a non-irritating alkali (lime water, milk of magnesia, etc.). Perhaps the writer insists rather strongly on this because a personal experience with strong formic acid leads him to think that it would be difficult to imagine anything more painful in application, more irritating in its local effect, or producing a more intractable form of ulceration.

All sorts of legends of the "Once upon a time" variety are current along the beaches and refer to some vigorous swimmer being incapacitated and drowned, sinking to the bottom at once as the result of being paralyzed by the sting of a powerful sea-nettle. Whether there is the slightest truth in any such rumor is an open question. The writer's own experience would tend to show that in a tide run or among waves the position of a swimmer's body is such that the hanging stinging tendrils of the animal hang below the swimmer and are not so very apt to come in contact, whatever the two bodies may do, and at the same time the movement of the water itself would carry the swimmer and the animal apart in a very short interval of time. On the other hand where or when the water was sluggish with slow current (or none) as in pools, eddys, salt-marshes, etc., the chances for prolonged contact and marked sting-results are greater. It may be that just because children play in such quiet waters that they seem to be the most usual victims.

It should not be forgotten that the poison of the sea-nettle is a paralyzer in one sphere of its action. The paralyzing effect is always there;

but as to there being any real danger to a child or adult all the writer can say is there is a lot of truth in a maxim of Arago's which states: "Aside from the realm of pure mathematics he who uses the word 'Impossible' lacks prudence. That the sea-nettle can sting is easily proved, the only question unanswered is the extent of the damage of which the animal is capable under circumstances that are favorable to the stinger.

In conclusion it might be well to mention that if an observer will but pass his hand beneath some floating jelly-fish he will be able to select from among the number those that sting and those that do not sting. Practically, a screen of so-called chicken wire, even though it be attached to floats by its upper margin and have a couple of feet or so submerged, will form a barrier against the entrance of jelly-fish to any given enclosure of water, that may be used for the amusement of children.



Book Reviews.

A Manual of Diseases of the Stomach. By WILLIAM MACLENNAN, M.B., Honorary Consulting Physician, Western Infirmary, Glasgow, etc.; with the assistance of J. SALISBURY CRANIG, M.B., Ch.B. London: Edward Arnold. 1921. First edition. New York: Longmans, Green & Co. 380 pages, with many illustrations. Price, \$7.50.

This author has aimed at a book midway between the small manual type and the large, pretentious work adapted chiefly to the specialist. On the whole he has succeeded pretty well in turning out a useful, comprehensive and well-arranged volume covering pretty much the field of gastro-intestinal disease.

The work is divided into four distinct sections, the first covering anatomy, physiology and chemistry, with clinical and laboratory methods; the second, organic disease of the stomach; the third, functional disease, and the fourth, stomach symptoms in relation to disease of other organs, with a glossary of chemical and clinical tests.

It is inevitable in a book of these pretensions that in the mind of any reviewer certain parts should be unduly elaborated at the expense of others, and here 33 pages on test meals as against approximately the same number of pages on a subject as important as gastric ulcer seems a little out of balance. Furthermore, a modern book without reference to the Sippy method of management in peptic ulcer is a bit unusual.

The real recommendation for the book lies in the fact that it contains in compact form between two covers a collection of generally sound, well-arranged information covering the field of gastro-enterology. The therapeutic side is tolerably well done, and prescriptions and diet lists are well elaborated.

This should prove a useful book for students and a convenient reference to practitioners.

Manual of Physio-Therapeutics, by Thomas Davey Luke, M.D., F.R.C.S., Ed. Formerly Physician, Peebles Hydropathic, Peebles, N. B., Assistant Physician, Smedley's Hydropathic, Matlock; Sometime Lecturer at the Edinburgh University: Author of "Spas and British Health Resorts," etc., etc. With many illustrations. New and revised edition.

"Would you have us place reliance
Less in drugs and pseudo science?
More in Nature; for our ills
Using sense and fewer pills:
No—the change were all too tragic,
Most folks like being healed by magic."—*Anon.*

New York: William Wood and Company, 1922.

Mr. Luke's "Manual of Physio-Therapeutics" is exactly what its name implies, including under the term "A Section on Diet in the Treatment of Disease," Thermo-therapy, Hydro-therapy, Masso-therapy, The Rest Cure, Electro-therapeutics and Diet-therapy are separately discussed in sections. The book is profusely illustrated and very readable, with frequent touches of quaint humor. The author discloses a very wide experience with the technical details of Physio-therapeutic methods employed in the European Spas, especially those of Great Britain. The description of apparatus and the technique of application will prove of great practical usefulness to the Physio-therapist. That he is a firm believer in the value of these is evident. The knowledge and review of contributory literature is extensive.

The book may be unconvincing to the pure scientist, and seem to the general practitioner perhaps too optimistic as to the great benefit to be expected in a wide variety of serious ailments, but the precautions and contra-indications are so clearly stated that no harm is likely to result.

The experience of the War has strongly suggested that a scientific application of these methods will probably result in an earlier return of function and in an alleviation of suffering in many types of disease and lesions.

We commend the book to the student or practitioner of physio-therapeutics. The general practitioner is likely to have his interest in this subject definitely stimulated.

THE BOSTON Medical and Surgical Journal

Established in 1882

Published by The Massachusetts Medical Society under the jurisdiction of the following-named committee:

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SUBSCRIPTION TERMS: \$6.00 per year in advance, postage paid for the United States, \$7.50 per year for all foreign countries belonging to the Postal Union.

Material for early publication should be received not later than ~~one month~~ for the ~~first~~ month, and sent to the printer with ~~policy~~ ~~proof~~ of paper. Upon written request, authors will be furnished free one hundred eight-page reprints, without covers, or the equivalent in pages in articles of greater length.

The Journal does not hold itself responsible for statements made by any contributor.

Communications should be addressed to The Boston Medical and Surgical Journal, 128 Massachusetts Ave., Boston, Mass.

THE LETTER OF HENRY D. NUNN, ESQ., MANAGER AND GENERAL COUNSEL OF THE MEDICAL LIBERTY LEAGUE.

THE JOURNAL is ready to give space to a courteous letter, but we feel that the explanation appearing in another column does not explain the charge that "most physicians take vaccination purely on faith" as the term is commonly used. If the critic will state that most physicians have a firm belief in the doctrine that vaccination by cow pox virus, so called, prevents and modifies smallpox the JOURNAL will agree with him, but if he uses the Bible definition of faith as "the substance of things hoped for, the evidence of things not seen," we shall continue to feel that the position and standing of the profession is not properly portrayed. Faith founded on study, observation and reliable testimony is the foundation of the practice of medicine. We do not speak slurredly of the decision of a court because the testimony presented in a trial deals with the statement of facts and opinions of those testifying, and no one should assume that physicians base attitude and procedure on general unsupported statements.

It is, of course, true that a medical student accepts the teaching of his professors but with the reservation of the right to form his own opinions after having acquired maturity of judgment founded on experience. Any assertion that tends to convey the idea that he continues to

have blind adherence to the teaching of his student days does not apply to the great mass of practitioners. Almost immediately upon entering practice he puts his instruction to test and accepts or rejects the doctrines taught, in accordance with experience and testimony. The great majority of medical students of today have trained minds and are eagerly searching for demonstrated truths. Medicine is becoming more scientific with startling rapidity and the inelastic and provincial minds of some practitioners of a generation ago are being rapidly eliminated. The younger practitioner must of necessity satisfy himself of the truth or falsity of the statements previously made and if he does not come in contact with smallpox, he finds that every epidemic of this disease spreads unless checked by vaccination. He finds that every up-to-date board of health and health officer having been in contact with smallpox emerges from the conflict endorsing vaccination. If he personally has the opportunity of seeing smallpox cases he finds that the teaching of believers in vaccination is sustained by his experience.

Skepticism of the merits of vaccination should be generally resented by the medical profession for the profession as a body believes in vaccination and physicians do not relish the implication that an accepted belief is false nor the many other statements which have been made from time to time imputing selfish motives and alleging the disproportionate dangers of vaccination. The illustration of the contention of the correspondent based on the attitude of the "erstwhile president of a medical association" is not apt, for although this surgeon had early in life formed an opinion it is evident that he had found no occasion for changing that opinion. Why should he keep informed of the technical details of the production of vaccine? He uses antiseptic and sterile material in his work but may be very much like many other surgeons who find no occasion to burden their minds with details of manufacture of these products. If he should find an abnormal percentage of sepsis among his cases he would find out the reason. Any physician who found smallpox as prevalent among the vaccinated as the unvaccinated would carefully study all factors involved and would confer with his associates with the purpose of finding the truth, i.e., if his experience led him to feel that the claims made of the efficacy of vaccination are false he would readjust his attitude.

The protective power of vaccination is demonstrable, we believe. If our friends of the opposition wish to make an effective study of the problem let them let them take one-hundred unvaccinated children and place them in contact with smallpox cases and, for control, subject one-hundred other children who have been successfully vaccinated to the same association. This matter can be dealt with in a way to convince any fair-minded person.

Recently some objectors to compulsory vaccination have simply argued that all that they wish is abolition of the *compulsory* feature of the law, but men with the same object in view are apt to be found associated and these less vigorous opponents of vaccination are found to be associated with those who earlier wanted to do away with vaccination altogether, and who talked about the spread of syphilis and the maimed and dead children following vaccination. If there are so many physicians who, if "candid," would admit having given no thought to the question of vaccination as claimed by the correspondent then there is a fertile field for the antivaccinationists to cultivate. We earnestly urge that this alleged great proportion of the medical profession be interviewed. It would be interesting to have them tabulated so that both proponents and opponents might know the approximate number of physicians who have no convictions on this subject.

The medical profession is under a solemn obligation to protect those in danger of disease who cannot protect themselves. If it was not for this duty the profession might be excused if the unpleasant features of controversy were abandoned. We are very like an army bearing succor to a threatened city; so far as we have ability, adherence to duty is demanded in the name of humanity.

No convincing arguments have yet been advanced which would justify retreat.

A FEATURE OF THE HARRISON NARCOTIC ACT.

In a decision of the Supreme Court written by Chief Justice Taft in denying a writ of error based on the charge that the indictment failed to charge that certain defendants sold inhibited drugs knowing them to be such, the court reversed the judgment of a lower court. The explanation is given that the statute does not make such knowledge an element of the defense. "Again where one deals with others and his mere negligence may be dangerous to them as in selling diseased food or poison the policy of the law may, in order to stimulate proper care, require the punishment of the negligent person though he be ignorant of the noxious character of what he sells."

In the opinion it is further stated, "The Narcotic Act has been held by this Court to be a taxing act with the incidental purpose of minimizing the spread of addiction."

This last exposition of the law sustains the interpretation which physicians have made. We are *taxed* for the right to use therapeutic agents. Because others make improper use of these agents is no valid reason why the Government, in all fairness, should impose a tax for revenue. All right minded physicians endorse all proper

methods designed to do away with the evils of drug addiction but when the Government taxes physicians engaged in ethical practice for the purpose of providing it with funds for protecting the people from harmful drugs it might go much further and levy a special tax on the surgeons' knives because some persons use knives in taking life. The registration of physicians with the right conferred to use narcotic drugs is logical, but the revenue tax is abhorrent and should be repealed. Doctors have contributed inestimable services to the nation and individuals and about all the public reward bestowed is exemption from jury duty. The degree of ability and industry exhibited by physicians, if applied in other walks of life, would average larger returns than those enjoyed by physicians and yet we must be taxed. Lawyers are not taxed to create a revenue which could be applied to controlling the shyster or any other class of criminals, but doctors are complacent and submit to exactions which are unjust.

As laws are enacted in the future the attitude and activity of the newly created legislative body of the A. M. A., will be watched with interest. We have not been able, as individuals, to protect ourselves. Will our representatives be more efficient?

NEWS ITEMS.

PROF. HARVEY CUSHING'S VISIT.—Prof. Harvey W. Cushing, who delivered the Cavendish lecture on Meningiomas last Tuesday evening before the West London Medico-Chirurgical Society, returned to America the next day, after acting for a fortnight as Director of the Surgical Unit at St. Bartholomew's Hospital. Some years ago when the Peter Bent Brigham Hospital at Boston, Mass., was founded in association with Harvard University, arrangements were made for exchange of visits by distinguished foreign surgeons, the visiting surgeon to be temporary surgeon-in-chief in place of Prof. Cushing for some period in each year. Prof. G. E. Gask deputised at Harvard in the spring of 1921, and Sir Cuthbert Wallace has just returned from filling a similar office. Although recognized as the leading neurological surgeon in the world, Prof. Cushing did not operate while he was in this country, but spent his time in observing methods of instruction, talking to students, and teaching in the out-patient and casualty departments of the hospital. His association with British surgeons during the war and the knowledge then acquired of the ways and manners of British sick and wounded made it possible for him to enter at once fully into the life of an English hospital. Our own surgeons envied Harvard its abundance of surgical assistants and its organization of type-

writing clerks and dictaphones to mitigate the labors of record keeping. It would be interesting to know what impression the great American surgeon formed from his intimate visit here. At all events, he has left a pleasant impression behind him.—*The Lancet*, 17th June, 1922.

WEEK'S DEATH RATE IN BOSTON.—During the week ending Aug. 19, 1922, the number of deaths reported was 196, against 194 last year, with a rate of 13.38. There were 40 deaths under one year of age, against 32 last year.

The number of cases of principal reportable diseases were: diphtheria, 66; scarlet fever, 12; measles, 24; whooping cough, 29; typhoid fever, 4; tuberculosis, 31.

Included in the above, were the following cases of non-residents: diphtheria, 4; scarlet fever, 4; typhoid fever, 1; tuberculosis, 9.

Total deaths from these diseases were: diphtheria, 5; scarlet fever, 1; measles, 2; whooping cough, 2; tuberculosis, 13.

Included in the above, were the following cases of non-residents: diphtheria, 1; tuberculosis, 1.

THE HEALTH ACTIVITIES OF THE UNITED STATES BUREAU OF MINES.—This report outlines the history and development of the Bureau, its legal authority, organization, personnel, appropriations and coöperation with other agencies. The report is the ninth in a series concerning those activities of the U. S. Government which deal directly or indirectly with the public health. The previous reports of the Council include, the Division of Vital Statistics of the U. S. Bureau of the Census; the Children's Bureau of the U. S. Department of Labor; the Women's Bureau of the U. S. Department of Labor; the Government Health Activities (with a chart); the Division of School Hygiene of the U. S. Bureau of Education; the Health Section of the Bureau of Indian Affairs of the Department of Interior; the Division of Welfare of the U. S. Post Office Department; and the Bureau of Animal Industry. It is planned in the near future to combine all of these reports in a single printed pamphlet, thus offering for the first time correlated, accurate and impartial data concerning the public health work of the Government. The National Health Council is a confederation of the fourteen leading national voluntary health organizations of the country, and has offices in New York and Washington. Copies of the Bureau of Mines Report or of any of the others may be obtained without charge from James A. Tobey, Washington Representative of the National Health Council, 17th and D Streets, N. W., Washington, D. C.

SCIENCE FOR AUGUST 8, 1922, PUBLISHES THE FOLLOWING NEWS ITEMS:

THE nineteenth annual meeting of the British Medical Association was held in Glasgow, Scotland, from July 25 to 28, under the presidency of Dr. David Drummond, of Newcastle-on-Tyne. Sir William MacEwen, Glasgow, was elected president for the year 1922-1923. Mr. Charles P. Childe, Southsea, is president-elect for the annual meeting to be held in Portsmouth in 1923.

DR. FREDERICK ROBERT ZEIT, for more than twenty years professor of pathology at Northwestern University Medical School, at his request has been relieved of active duty in the medical school. He plans to spend next year abroad. The pathological museum of the university will hereafter be known as the Frederick Robert Zeit Museum of Pathology.

DR. GEORGE MILBRY GOULD, known for his work in medical ophthalmology and especially in eyestrain, formerly editor of *American Medicine*, *Biographic Clinics* and the Gould Medical Dictionary, died on August 8, aged seventy-four years.

THE third International Congress of the History of Medicine was opened on July 17, at the Royal Society of Medicine, London. Dr. Charles Singer, lecturer on the history of medicine, London University, presided. The following countries were represented: Belgium, Czechoslovakia, Denmark, Egypt, France, Greece, Holland, Italy, Portugal, Rumania, Spain, Switzerland and the United States. Lord Onslow, parliamentary secretary to the Ministry of Health, welcomed the delegates on behalf of the government, after which Dr. Singer addressed the congress. Dr. Laiguel-Lavastine acknowledged the welcome on behalf of the foreign delegates.

Mission and educational bodies of East China have set in motion a project to build in Shanghai a union medical school at a cost of \$500,000. St. John's University of Shanghai, which now has a medical department, is one of the institutions supporting the project.

CHANGE OF OFFICE.—Dr. David D. Seannell has removed his office to 320 Commonwealth Avenue, Boston.

Miscellany.

LEGISLATIVE MATTERS

The National Health Council in its report covering August 1-14, relating to legislation contains the following:

The Tariff and Public Health. H. R. 7456.

“The fixing of the final vote on the Tariff Bill

on August 19th, brings up the question as to what will be done on the embargo provisions therein relating to dyes and the related synthetic drug industry. Before the War the United States produced none of these materials and all came from the German chemical sources. Due to necessities created by the War, several hundred plants and laboratories have developed in America, producing Salvarsan, Cinchophen, and Novocaine and other important medicinals. On a recent vote the Senate refused to retain the present embargo against the importation of such products. The medical fraternity throughout the country, and also chemists and laboratories, are demanding that Senators reverse their vote and continue the embargo. A second vote will be had. If the embargo is not retained it is claimed that at least two hundred laboratories and plants producing synthetic medicines will not be able hereafter to meet competition from German sources which will have entry into the United States."

H. R. 4.

"This bill, which passed the House on February 1, 1922, and passed the Senate, August 3, 1922, grants pensions to army nurses who served under contract with the Army during the War with Spain, Philippine Insurrection, or Chinese Boxer campaign for 90 days, or who were disabled and released prior to 90 days' service.

"When the Army Nurses Corps was first organized in 1898, nurses were employed under contract by the Surgeon-General of the Army. Under the existing Army Reorganization Law of February 2, 1901, nurses are given a military status from and after that date. The present law extends this recognition so as to give a military status to Army Nurses beginning with the Spanish War and running to February 2, 1901. The effect of this bill will be to give women who served as nurses the exact pension allowances that are awarded to soldiers in the War with Spain, etc., under the act of June 5, 1920."

AMERICAN RELIEF ADMINISTRATION.

Moscow, July 13.—If medicine could cure the body politic, Russia by now ought to be on a fair way to recovery. The American Relief Administration is pouring medicine into the Soviet Republic, not by the teaspoonful, but by the ton.

If purgation has a corresponding effect upon Russian red that fiery hue ought soon to be reduced to at least a pale pink, for one of the items on the list of surplus army medical supplies sent to Russia is 52,000 pounds of Epsom salts. No one who served with the A. R. A. in France would quite believe it possible considering the number of "C. C." pills consumed by the American Army, but it is a fact that the

surplus sent to Russia is 50,000,000. Epsom salts used with discretion yield some 64 doses to the pound. Fifty-two thousand pounds of Epsom salts would mean 3,328,000 doses. Castor oil is another big item on the laxative list, and there are others.

The quantities are stupendous, but so is the need. Hospital after hospital, dispensary after dispensary has found itself unable to compound the simplest prescriptions because of the lack of many common and generally used pharmaceutical preparations.

Malaria, which is becoming increasingly prevalent in Russia, has raged practically unchecked because quinine was almost unobtainable. America's contribution of quinine is 20,000 pounds.

If Russia should cut its finger there would be no immediate lack of dressings. In the first place, if the cut is not a serious one, there are 80,000 bottles of collodion, to be applied after half a million tubes of iodine had been used to cleanse the wound, and 1,200,000 iodine swabs had been used to wipe it out. If it were more serious, there would be bandages enough and to spare. The A. R. A. has received for distribution 21,600,000 plain gauze bandages and more than 6,000,000 yards of compressed gauze bandages and over half a million pounds of cotton.

The A. R. A. order for neo-salvarsan, which is regarded as a perfect specific for typhus recurrents as well as for syphilis, to date totals nearly half a million tubes.

In addition to the soap distributed through the regular channels in connection with child feeding and general relief, the medical department of the A. R. A. has had close to 1,000,000 pounds to give out to hospitals, homes and institutions.

The 400,000 pounds of sulphur is being distributed.

RÉSUMÉ OF COMMUNICABLE DISEASES JULY, 1922.

GENERAL PREVALENCE.

There were 4,097 cases of communicable disease reported during July—2,734 less than the total for the previous month. The decrease was noted in all of the more common communicable diseases, with the exception of whooping cough, and is the usual decrease for this season.

Anterior Poliomyelitis.—There were 23 cases of this disease reported during July. This shows the usual increase during the month of July, but does not equal the total of 26 cases obtained during July, 1921. The cases as reported are scattered throughout the eastern

section of the State, the only city showing an unusual number of cases being New Bedford, with a total of 7.

Chicken-pox was reported in 136 instances, a sharp drop from 325 cases reported during June.

Diphtheria shows a slight decrease from 459 cases reported during June, the month's total being 381. This disease continues to be reported in slightly fewer numbers than last year.

Encephalitis lethargica was reported in 6 instances.

Epidemic cerebrospinal meningitis was reported but once during the month. While a falling off in the number of cases is usually noted during the summer months, this low figure has not been reached for several years.

Gonorrhœa and syphilis were reported in 407 and 153 cases, respectively.

Malaria was reported in 8 instances, an increased number of reports to be received.

Measles continues to be reported in large numbers, 1,275 reports being received, as compared with 866 for the same month of last year. This, however, is a decided decrease from the 3198 cases reported during June.

Mumps is also showing a decrease in reports received, dropping from 375 cases in June to a total of 184 in July.

Scarlet fever was reported in 226 instances, about the average for this month.

There were 506 cases of *pulmonary tuberculosis* and 69 cases of *tuberculosis, other forms*, reported. This is a decrease from the previous month.

Typhoid fever increased from 41 cases in June to 74 in July. The only communities showing an unusual incidence were Worcester, with 10 cases, and Montague, with 3 cases.

Whooping cough increased in the number of cases reported to 413. While an increase of 54 over the previous month is not unusual, it is conspicuous because this is the only one of the so-called common diseases which has increased rather than decreased during the month.

RARE DISEASES.

Anterior Poliomyelitis was reported from Attleboro, 1; Boston, 2; Brookline, 1; Cambridge, 1; Clinton, 1; Fall River, 1; Haverhill, 1; Lawrence, 1; Medford, 1; Methuen, 1; New Bedford, 7; Revere, 1; Somerville, 2; Uxbridge, 1; Westfield, 1; total, 23.

Dog-bite requiring anti-rabic treatment was reported from Barnstable, 2; Boston, 9; Cambridge, 1; Chelmsford, 3; Georgetown, 1; Holyoke, 1; Lowell, 15; Medford, 1; Plymouth, 1; Springfield, 1; Wellesley, 4; Weston, 1; Worcester, 1; total, 41.

Encephalitis lethargica was reported from

Boston, 2; Cambridge, 1; Peabody, 1; Springfield, 1; Worcester, 1; total, 6.

Epidemic cerebrospinal meningitis was reported from Fall River, 1.

Hookworm was reported from Boston, 2.

Malaria was reported from Boston, 2; Fall River, 1; Haverhill, 1; Milford, 3; Northampton, 1; total, 8.

Pellagra was reported from Northampton, 1.

Septic sore throat was reported from Boston, 2; Fall River, 1; Leominster, 1; Norwood, 1; Springfield, 1; Whitman, 1; total, 7.

Tetanus was reported from Fall River, 1.

Trachoma was reported from Boston, 4; Fall River, 1; Fitchburg, 1; Lowell, 1; Somerville, 1; total, 8.

DISTRIBUTION.

All communicable diseases: Total cases (all causes) for July, 1922, 4,097; for July, 1921, 3,989. Case rate per 100,000 population for July, 1922, 104.1; for July, 1921, 102.2.

Certain prevalent diseases: Diphtheria—Total cases for July, 1922, 381; for July, 1921, 441. Case rate per 100,000 population for July, 1922, 9.7; for July, 1921, 11.3. Measles—Total cases for July, 1922, 1,275; for July, 1921, 866. Case rate per 100,000 population for July, 1922, 32.4; for July, 1921, 22.2. Scarlet fever—Total cases for July, 1922, 226; for July, 1921, 251. Case rate per 100,000 population for July, 1922, 5.7; for July, 1921, 6.4. Typhoid fever—Total cases for July, 1922, 74; for July, 1921, 62. Case rate per 100,000 population for July, 1922, 1.9; for July, 1921, 1.6. Whooping cough—Total cases for July, 1922, 413; for July, 1921, 474. Case rate per 100,000 population for July, 1922, 10.5; for July, 1921, 12.1. Tuberculosis, pulmonary—Total cases for July, 1922, 506; for July, 1921, 479. Case rate per 100,000 population for July, 1922, 12.9; for July, 1921, 12.3. Tuberculosis, other forms—Total cases for July, 1922, 69; for July, 1921, 55. Case rate per 100,000 population for July, 1922, 1.8; for July, 1921, 1.4.

Correspondence.

INSTITUTE OF PREVENTIVE MEDICINE

Mr. Editor:

I am enclosing a free translation of an article by Professor Laurent of Nice, which was read before the Paris Academy of Medicine. In view of the interest now taken in arranging a National Health Examination Day, this added proof of the acceptance by progressive medical men throughout the world of this principle of periodic examination is important.

Yours very truly,

EUGENE L. FISK, M.D.
Medical Director, Life Extension Institute, Inc.
25 West Forty-Fifth Street, New York.

*Translation.***THE INDIVIDUAL HEALTH RECORD AND THE INSTITUTE OF PREVENTIVE MEDICINE. BY PROFESSOR O. LAURENT, OF NICE.**

Correspondance Member of the Paris Academy of Medicine; Laureate of the Paris Academy of Sciences; Ex-Major (1st class) of the French Army; Honorary Member of the Academy of Medicine of Rio-de-Janeiro, (*The Bulletin of the Academy of Medicine, Paris, No. 24, June 13, 1922.*)

THE THERE are two provinces of medicine, therapeutic and preventive. The latter, until recently given very little attention, is assuming an importance equal to that of the former, to the extent even that its services are becoming invaluable. Can one compute approximately the number of deaths from typhoid fever avoided during the war, and the number of complications that the epidemics of that disease would have caused, depreciating the life cycle? However, if the length of life has thus increased on the whole, the principal factor in this change is the lowering of infant mortality, so that we may say that the adult still actually remains insufficiently protected against menacing disease (the causes of morbidity-mortality being: toxemias, physical and psychical traumas and apathies, food deficiency and hormone deficiency) for, in spite of numerous efforts on every side, except when forced to do so, it is quite exceptional that man has himself medically examined solely for the purpose of finding out his physical condition, to perfect his hygiene, and to avoid illness. On the other hand, the examination taken solely for the purpose of conserving one's health, ought to be made under certain conditions, if it is to become universal and answer to all the desiderata of science.

There is need to establish a suitable organization with co-ordinated activities.

The *Institute of Preventive Medicine*, of the conservation of health, of the prevention of sickness, would comprise regional centres for the periodic physico-chemical examination, very easy of access, available to every adult, not ill, affiliating himself with the Association. The realization of this work would be easy because there exist in all the French towns x-ray and chemical laboratories, and the expense that they would incur would be covered by the thousands of individual or collective (industrial workers) subscribers.

Specialists should therefore form themselves into regional groups for the biological physico-chemical examination at the disposition of the member, for whom they will make out a balance-sheet of the complete physical condition, and to whom they will give a health bulletin together with general necessary cautions, especially with regard to hygiene, but without interfering in any way with the diagnostic clinic and with treatment.

These centres of physical examination would be connected with the General Committee, from which they would receive directions and which would have its journal. For propaganda they would rely upon the medical profession and the Red Cross; it could be associated with other works, anti-cancerous and anti-tuberculous, so beneficial to the public.

The health examination would often show up the functional troubles which are predecessors of definite illness as well as a number of latent ills which are advancing towards the stage of gravity. How many men and women reach the age of 50 without ever having undergone a complete examination, afflicted however for some time with diseases of the heart, the blood vessels and the kidneys, or with various diseases already in an advanced stage! The general practitioner every day deplores the consequences of negligence, ignorance, quackery, prejudice, which only too often prove to be fatal. A great blank to make good!

In the United States the Life Extension Institute, which was organized years ago under the chairmanship of Mr. Taft, former president of the Republic,

comprises 7,000 physicians who have examined 250,000 people. (The privilege is extended to more than 600,000 insurance policyholders.) Dr. Eugene Lyman Fisk, Medical Director of the Institute, has published a number of works on this question; in collaboration with Professor Irving Fisher he is the author of the book, "How to Live." 25,000,000 in the working classes have mouth infection or defective teeth. It is reckoned that 80 per cent. of the deaths among adults (through infections of the heart, blood vessels or genito-urinary organs) could have been postponed for periods ranging from a few days to several years. And if this year 750,000 people are going to die from preventable diseases, we may conclude that France this year will suffer the loss of 200,000 in this way.

Intended principally for people who have reached the age of 40 and are physically fit, or believe themselves to be so, the physico-medical examination will bear principally on the condition of the heart, the blood pressure, the blood and the urine, but it will be absolutely complete and will take up scientifically the condition of all the organs.

Thus medical science, comprehensive as it is, the Encyclopaedia of Practical Medicine, would constantly be able to penetrate into the smallest corner of Brittany or of Savoy, as well as it can in the Paris hospital. The member would have not only the best chances of conserving his health and lengthening his life, but also in case of sickness or accident befalling him no matter how, of being cared for more logically, the doctor treating him having at hand the invaluable guide of a complete examination made previously. And how many cases of diabetes, of arteriosclerosis, of tuberculosis and of cancer would be discovered in time! The large public or private organizations and the insurance companies would do well to make general the periodic preventive examination.

By the quantity and the variety of the documents that it would gather from all parts and the studies to which it would be able to devote itself, this Institution would constitute a *true and extensive practical laboratory of life*, being especially useful in determining precisely the *medical constitution of a district*, its deficiencies and the remedies that should be applied to it. Unfortunately, we must reckon with the general indifference and custom, but we ought not to forget that the sphere of social hygiene is most extensive, unlimited one might say.

I hope and believe, however, that if a group of persons co-operated with the Association with the same zeal that Professor Hartman and Professor Delbet co-operate in the work for the prevention of cancer, the Institute of Preventive Medicine would be able, by the side of other organizations, to make some contribution to public health and to the optimum of efficiency. Social hygiene will have to undergo a great evolution, a thorough revolution of its operations.

THE REPORTING OF TYPHOID FEVER.

CITY OF BOSTON, HEALTH DEPARTMENT.

August 22, 1922.

Mr. Editor:

May I have the privilege of your columns to call attention to a matter which is of serious importance to the public, especially at this season of the year, and which should receive the consideration of the medical profession?

During the past few years it has become a more common occurrence than formerly for the Boston Health Department to discover nests of typhoid fever in the city in one of the following ways:

A death certificate signed by a physician who gives the cause of death as typhoid fever is presented to the Health Department and a burial permit requested. It is at once noted that the case of typhoid fever had never been reported to the Health Department as required by law. In such an instance, the physi-

cian will usually be found to be on the staff of one or another of the Boston hospitals and investigation will show that the deceased has been a patient in the hospital perhaps from one to four weeks and that his temperature chart for the first 48 hours after his admission was alone sufficient to justify a suspicion of typhoid fever. Investigation at the residence of the deceased will disclose other unreported cases of typhoid fever, sick or convalescent, in the family, a physician in attendance, often previous attendance by other physicians, and a history of a household spread of typhoid fever which clearly points to successive contact infections of one member of the family after another.

Or it may be that it is not a hospital but a private practitioner who may finally report typhoid fever in a family which has long been under his care.

When called on to explain failure to report cases of typhoid fever promptly to the Health Department, both hospitals and private practitioners are accustomed to plead difficulty of diagnosis with a good deal of talk about inability to get positive Widal reactions. How this plea may be worked is shown in the following specific instance:

On August 4 a local hospital reported the recent admission of a child ill with typhoid fever. This report led to the discovery by the Health Department that the family had been ill with typhoid in their home since May. This first case developed the first of May in a boy who had apparently picked up the disease somewhere away from home. About two weeks later the father, who was employed in the milk business, came down with the disease. The mother, another boy and then, finally, the little girl who was sent to the hospital, were successively stricken. The family were attended in their home from the beginning by a physician who enjoys a good professional reputation and who has a large practice. He states that clinically the cases were all typical typhoid fever from the first, but that repeated specimens sent into the Boston Health Department Laboratory were reported negative as to a Widal reaction. A prominent local physician was called in as a consultant and as a result of his advice that the attending physician was not justified in calling the cases typhoid fever in the absence of a positive Widal reaction, the Health Department was not notified of the cases, in spite of the evidence of the communicability of the disease. It may be stated that the records of the Health Department Laboratory show that at an interval after the onset of the disease sufficient to lead one to expect a positive Widal reaction, specimens were submitted to the Laboratory in the cases of three members of the family and that the Laboratory returned negative reports.

For various reasons above mentioned the Health Department is discovering when it is too late that with a physician in attendance from the start, one member of a family after another has become infected with a disease clinically indistinguishable from typhoid fever. This has happened not merely once but repeatedly. For the reasons mentioned, in a city which boasts of its opportunities for a medical education, the Health Department does not know how many unreported cases of typhoid fever there may be in the city at the present time and to what extent the health of the people may thereby be jeopardized.

A municipal health department finds it a part of its regular work to quietly clean up messes resulting from diagnostic errors in hospital and private practice and it would appear from the daily experiences of this office that there are two important principles which present-day medical education is failing to teach. One is that the spread of a communicable disease is not going to be checked if one waits until a positive diagnosis can be made before taking effective means to prevent its spread. It makes no

difference whether clinical symptoms or laboratory reports are relied on to establish a diagnosis. It also makes no difference whether the disease be typhoid fever, smallpox, diphtheria, scarlet-fever, measles or whooping-cough, or any other communicable disease.

The other fundamental which our daily experience leads us to believe to be not sufficiently appreciated is that when a laboratory report is inconsistent with the clinical evidence in any case, the clinical evidence should not be discarded, but the laboratory report questioned. If a physician will make a regular practice of sending specimens taken at the same time and in the same manner to four different laboratories, he will find that he will receive a certain proportion of inconsistent laboratory reports, whether the specimens be blood for the Widal or the Wassermann reaction, smears for evidence of gonorrhoeal infection, or swab cultures for Klebs-Loeffler bacilli, or specimens submitted for any purpose whatever. It may be that inconsistent or erroneous reports are the fault of somebody in the laboratory, or of the physician who sends in the specimen, or perhaps a consequence of the falsity of some fundamental hypothesis on which the laboratory procedure, or even present-day bacteriology itself, has been built up, but the practical fact always to be reckoned with is that laboratories do return negative reports when positives are to be expected and also return positive reports when, on theoretical grounds, negatives are to be expected. To those who have acquired the habit of looking to the laboratory as a means of diagnosis, instead of as an aid to diagnosis, the procedure above referred to is recommended—the practice of always sending portions of the same specimen to four different laboratories. The results of such procedure will at least tend to jolt a habit of blind reliance on laboratory reports.

Very truly yours,

F. X. MAHONEY,

Health Commissioner.

M. VICTOR SAFFORD, Deputy, Medical Division.

SMALLPOX TREATMENT AND FACTS RELATING TO VACCINATION.

Mr. Editor:

In your issue of August 10, I note with interest Dr. Pasquale Romeo's paper on the treatment of smallpox by the violet ray.

Since coming to Ohio five years ago, I have had the opportunity of studying approximately 2,000 cases of smallpox, many of them confluent on the face and involving the conjunctiva and the mucous membranes. At our isolation hospital, practically the only treatment used was to keep the bowels flushed out and to apply vaseline to the lesions. These cases, of course, were almost invariably diagnosed as influenza and correctly diagnosed only on the appearance of the rash, hence they were always in the pustular stage when admitted. I can recall no case which showed any scarring beyond a few very slight ones on the face. It may be that the violet ray is capable of preventing pustulation: Dr. Oscar Hayes, formerly Health Commissioner of Denver, tells me that they always put their patients out in the sunlight.

Referring also to your issue of Aug. 17, p. 270, I should like to add that out of the hundreds of cases examined by me as Epidemiologist here, only 2 presented the scar of a successful vaccination. About 5 more had scars typical of a secondary mixed infection on the site of the vaccination. The others had never been successfully vaccinated.

Respectfully,

Malcolm Dean Miller, M.D. (formerly a Fellow),
Akron, Ohio, August 23, 1922.

THE STING OF THE SEA-NETTLE.

Mr. Editor:

It occurred to me that possibly this portion of a letter, that has just been written to me by a well known editor, might be of some slight interest to you. As you can see he states: "Is it not queer that we cannot find anything about the sting of the sea-nettle in the books? We know that people have been stung by those animals long before the present summer but why have not the authors had something to say about those things in their writings? I think I have read reports of such cases in some current journals but treatment, if any has been given, has never got out of those journals and into any more or less permanent books of reference. If the BOSTON MEDICAL AND SURGICAL JOURNAL does publish your article be sure that I get a copy of the issue in which it appears, for I wish to speak of it editorially and to call attention to the fact that after going through the indices of almost 1000 books, I have not been able to find even a mention of the sea-nettle, much less to find any comment on its stinging powers or on the treatment of the conditions brought about by its sting. The only discoverable reference is in the Century Dictionary, which states that the sting of the sea-nettle causes urticaria and asks why no authorities on dermatology have ever made mention of such a skin condition, its character or its treatment. I can say that the works on therapeutics wholly overlook this particular thing in the portion of such books that is devoted to specific therapy. I may add that even the great and only Janning (*Jour. A.M.A.*) has wholly overlooked this apparently rather common matter of sea-nettle stings. Perhaps its abstractors have been asleep at the switch."

Douglas H. Stewart, M.D.
128 West 86th Street, New York.

CERTIFICATE OF REGISTRATION LOST.

Mr. Editor:

Dr. Herman Augustus Tyler has, this day, reported that his Massachusetts certificate of registration as a practitioner of medicine, license number 11,846, was left by him on a table in the Hotel Bancroft, Worcester, Mass., and he has been unable to find the same although he wrote to the hotel at once, relative to the loss.

Very truly yours,

SAMUEL H. CALDERWOOD, M.D., Secretary.

THE RADIO TALK ON VACCINATION.

Mr. Editor:

Some of my good friends are members of the medical profession, and are doubtless readers of your excellent JOURNAL. It would distress me to have them suppose that I had spoken disrespectfully of physicians or in any way deserving to be characterized as with "coarse humor," in my radio talk on vaccination discussed by your editorial in the August 17 issue. I would, therefore, appreciate the courtesy of a little of your space to explain.

When I said that "the great majority of those who believe firmly in vaccination, including most physicians, take vaccination purely on faith, without giving the subject any real thought," I did not intend to be either humorous or offensive.

Physicians, having to employ in their work such a vast number of therapeutic and prophylactic agents, as they do, must necessarily take not a few of them "purely on faith." I believe no physician who may be given the opportunity to read this, will deny that such is the fact.

It is too bad that skepticism as to the merits of vaccination should so generally be resented by the medical press, as opposition to the medical profession. The practice of vaccination does not figure conspicuously in the work of the average physician and with so many pressing problems to engage his thought, he has no personal incentive to delve into the history and fundamental facts relating to vaccination, which he was taught in college to believe was an unquestionable boon to the race.

I am sure that most physicians who are candid with themselves will admit that they have always considered vaccination as something requiring no thought by reason of its long acceptance by the profession.

Quite recently, I had some friendly correspondence with an eminent physician, erstwhile president of the medical association of his State, concerning vaccination. This gentleman made certain statements which indicated that he had not kept abreast of the recent developments in the product used as vaccine virus, and when I asked further questions he frankly admitted that he could not inform me; that his work was exclusively confined to surgery and that he had not vaccinated anyone for many years. Nevertheless he was thoroughly dogmatic in his support of the practice of vaccination.

I believe that it is a serious mistake for the medical profession to elect to stand back of vaccination as now practiced, as a worthy, if not preeminent example of the advancement of medical learning.

As to the article by Dr. Heiser in the *Journal of the A. M. A.*, to which you refer, I believe any unprejudiced student, be he physician or layman, who studies the official Reports of the Philippine Health Service for the years 1918, 1919 and 1920, will be forced to conclude that the doctor must have written his article hastily, for it discredits itself by crude inaccuracies and by illicit arguments, which I will be glad to point out if you care to give me the space in which to do it.

Sincerely yours,

HENRY D. NUNN,
Manager and General Counsel, Medical Liberty League, Inc.

REPRINTS.

A few reprints of The Treatment of Diabetes Mellitus, by Elliott P. Joslin (*Bos. Med. and Surg. Jour.*, June 22, 1922), are available and may be procured by applying at this office. Price 50¢.

BOSTON MED. AND SURG. JOURNAL,
126 Mass. Ave., Boston.

MASSACHUSETTS BOARD OF REGISTRATION
IN MEDICINE.

EXAMINATION HELD JULY 11, 12, 13, 1922.

Applicants Examined	164
Applicants Registered	138
Applicants Rejected	25
On Table	1

DETAIL OF COLLEGE OF GRADUATION, AS FOLLOWS:

Tufts College Medical School.....	56
Harvard Medical School.....	26
Middlesex College of Medicine and Surgery.....	22
Boston University School of Medicine.....	18
St. Louis College of Physicians and Surgeons.....	9
Massachusetts College of Osteopathy.....	6
College of Physicians and Surgeons, Boston.....	5
Chicago College of Osteopathy.....	3
Kansas City University of Physicians & Surgeons	2
Hahnemann, Philadelphia.....	2
University of Vermont.....	2
University of Southern California.....	1
University of Budapest.....	1
University of Georgetown.....	1
American Medical Missionary College.....	1
State University of Iowa.....	1
University of Maryland.....	1
Temple University.....	1
University of Michigan.....	1
Ohio State University.....	1
McGill University.....	1
Rush Medical College.....	1
Columbia University College Phys. & Surg.....	1
Imperial Ottoman Medical College.....	1
Total	164

THE COLLEGE OF GRADUATION OF REJECTED APPLICANTS
IS AS FOLLOWS:

Middlesex College of Medicine & Surgery.....	9
St. Louis College Physicians & Surgeons.....	8
College of Physicians and Surgeons, Boston.....	4
Massachusetts College of Osteopathy.....	2
Tufts College Medical School.....	1
University of Southern California.....	1

Total	25
(University of Budapest, on table.)	

SPECIAL NOTICE

The Boston Medical Library, permanent headquarters of The Massachusetts Medical Society, 8 The Fenway, Boston, desires contributions of books, periodicals, pamphlets, medical photographs and autographs, and whatever relates to medicine. Do not throw away or sell for junk anything of a medical or scientific nature, no matter how worthless it may seem, without first giving the Library the privilege of examination.

COMMITTEE IN CHARGE OF PLANS FOR A
NEW ENGLAND MEDICAL MEETING.

PRESIDENT JOHN W. BARTOL has appointed Dr. A. P. Merrill of Pittsfield and Dr. W. P. Bowers of Clinton to act with him in planning for joint medical meetings of the New England state medical societies.

INFANT FEEDING.

By an Anonymous Member of the
American Pediatric Society

LINES SUGGESTED BY THE PAPERS ON INFANT FEEDING

Soranus, he of ancient Rome,
He had a simple trick
To see if milk was fit for sale,
He merely dropped it on his nail
To see if it would stick;
Yet spite of this the babies grew
As any school boy'll tell to you.

Good Metlinger in ages dark
Just called milk good or bad
No acid milk could vex his soul
He gave it good, he gave it whole
A method very sad;
Yet babies grew to man's estate
A fact quite curious to relate.

Time sped and science came along
To help the human race,
Percentages were brought to fame
By dear old Rotch, of honored name,
We miss his kindly face;
Percentages were fed to all
Yet babies grew both broad and tall.

The calories now helped us know
The food that is required
Before the baby now could feed
We figured out his daily need
A factor much desired;
Again we see with great surprise
The babies grow in weight and size.

The vitamin helps clarify
Why infants fail to gain,
We feed the baby leafy food
Which for the guinea-pig is good
A reason very plain;
And still we watch the human race
Go madly at its usual pace.

We have the baby weighed today
The nursing time is set,
At last we find we are so wise
We can begin to standardize
No baby now need fret;
In spite of this the baby grows
But why it does God only knows.

Away with all such childish stuff
Bring chemists to the fore,
The ion now is all the rage
We listen to the modern sage
With all his latest lore;
And if the baby fret or cry
We'll see just how the ions lie.

A hundred years will soon go by
Our places will be filled
By others who will theorize
And talk as long and look as wise
Until they too are stilled;
And I predict no one will know
What makes the baby gain and grow.